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## CLINICS.

### Clinical Lectures.

### CLINICAL LECTURE ON DISEASES OF WOMEN,

DELIVERED AT THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

BY T. GAILLARD THOMAS, M.D.,

PROF. OF THE DISEASES OF WOMEN IN THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

[Reported by P. Brynberg Porter, M.D., and Revised by the Author.]

CASE I.—*Very Marked Anteflexion of the Uterus, giving rise to Nocturnal Incontinence of Urine.*

GENTLEMEN: This patient, whose name is Mary R——, is twenty years old, a native of the United States, and single. Her case would not ordinarily be considered of much consequence; but I regard it as a very important one for us, because it illustrates so perfectly the necessity of making a physical examination whenever anything in the history points towards the pelvic viscera. The chief symptom, and there is apparently only one of any consequence here, is a very curious one, and it would otherwise remain a complete mystery; while, in the light of such an explanation, it becomes a perfectly simple matter. She states that she has had the trouble of which she is now complaining for seven years, or ever since she was thirteen years old, which is about the time she began to menstruate. Now this is her trouble. She is able to retain her water perfectly all day, but at night when she goes to sleep, it passes from her involuntarily. This nocturnal incontinence of urine is common enough in children, the bladder contracting, and the urine thus escaping after they retire to rest, but it is a very unusual thing to find it in an individual of twenty. The only other things that the patient complains of are, a "feeling of weakness through the body" when she makes any effort, and very severe headache after stooping over. These are obscure symptoms, and need not occupy our time; but the matter of the incontinence of urine is a palpable trouble, and it is for this that the young woman comes here to seek relief.

If such a patient should present herself at the office of any one of you, and you should proceed to treat her without making a vaginal examination,

you would probably prescribe belladonna, and perhaps electricity in addition, and you would be very careful to have her wake at certain intervals during the night in order to evacuate her bladder in the natural way. After a prolonged course of some such treatment, you would probably begin to wonder why she did not get any better; but if you should then make an exploration of the pelvic viscera, the mystery would at once be solved, and the whole case be made perfectly clear.

Let me tell you, then, what was revealed by the examination which I made here. Although the patient said that she had not passed urine for five or six hours, I found the bladder quite empty, apparently; and this was proved to be the fact when I passed the catheter. The posterior wall of the bladder was pressed firmly against its anterior wall by means of a tumour, which I ascertained to be the body of the uterus in a state of extreme flexion. By conjoined manipulation I was able to recognize and to rock the uterus in such a manner that the diagnosis was complete without the introduction of the sound.

But there is something more about the case which has not yet been alluded to, and that is dysmenorrhœa. The patient stated to me in private that she always suffers intensely at the time of her monthly periods, although she did not mention this when I was questioning her before the class in regard to her symptoms. There are, then, two symptoms of importance which claim our attention in this case, viz.: nocturnal incontinence of urine and dysmenorrhœa; and let us see if the condition of affairs revealed by the examination will satisfactorily account for these.

As to the dysmenorrhœa it is not difficult to understand how that should be one of the symptoms here. The menstrual blood becomes dammed up at the *os internum*, the seat of the flexure, and this excites violent contractions of the uterus to expel it. Presently a coagulum is driven out, and then for a short time the blood finds exit, after which the same thing is repeated, causing the patient a good deal of suffering, until the menstrual period comes to an end. In regard to the incontinence of urine it may either be caused by the irritability of the bladder, from the constant pressure exerted upon it, so that it refuses to retain urine after a certain limit; or it may be that while volition remains the patient is able to resist the tendency to evacuate the bladder to which the pressure upon it gives rise, and that it is only when sleep comes on and the nervous system loses its control of the viscus that it is unable to retain it any longer. But, at all events, there can be no doubt that the incontinence is directly attributable to the anteflexion of the uterus, whose body thus presses with such force and so uninterruptedly upon the bladder.

But, it may be asked, why does not lying upon the back remove the pressure of the uterus from the bladder? Under ordinary circumstances this would be the case; but here the anteflexion is so aggravated that instead of relieving the bladder it only increases the difficulty. The bladder seems to be affected by a kind of spasm, and you know how apt spasm is to come on during sleep. This is the case with spasmodic croup, and spasm of the neck of the bladder, as a rule, comes on just after the patient has awakened from sleep.

Having thus discovered that it is the direct pressure of the flexed uterus upon the bladder that gives rise to the nocturnal incontinence of urine, what is required for the cure of the latter? The answer is very simple: Lift the body of the uterus off of the bladder, and then retain it in its normal position. But although it is very simple to say, "Lift the uterus

off the bladder," this is sometimes a very difficult thing to do. In the present instance, however, I think it can be accomplished quite readily. The restoration should be accomplished gradually, or very serious trouble may be set up. Placing the fingers of one hand in the vagina, and those of the other on the abdomen, the fundus should be raised but a short distance at first, after which a little pad of cotton may be inserted to retain it in its new position. The same procedure should be repeated over and over again; and after a time, when the organ can be completely restored to its normal place, a properly constructed and nicely-fitting anteversion pessary should be applied, and worn as long as necessary. No medicine whatever would be of the slightest efficacy here, because the trouble is to be looked upon as a purely mechanical one, and I am confident that if we succeed in meeting the indication here presented we will cure the patient; while if we do not meet the indication, no amount of medication can possibly do her any good. This is pre-eminently a case for the hospital, as it is almost impossible to treat such a case in this young woman's walk of life satisfactorily elsewhere; and I trust that she will consent to enter my service at the Woman's Hospital.

CASE II.—*Large Uterine Fibroid; Chronic Ovaritis.*

Our second patient to-day is Margaret W——, thirty-six years of age, and a native of Germany. She has been married fourteen years, and has never had any children, though she had one miscarriage twelve years ago.

Of what do you complain, Mrs. W——?

"Pain in the back."

How long have you had it?

"About two years."

What else?

"When I walk any distance, or do much work, I get very faint, and feel as if somebody had beaten me all over with a club."

Do you ever faint away?

"Yes, sometimes I faint away entirely."

What else do you complain of?

"I do not get my turns as I ought to."

What is wrong about them?

"They do not come at the regular time always, and they do not last long enough."

Do you have much pain at the time of your monthly sickness?

"Yes, I suffer very greatly."

What else do you suffer from?

"Headache."

What else?

"Pains in my shoulder."

What else?

"That is all."

Nothing, gentleman, I assure you, can take the place of clinical teaching, because, at a clinic, each student has the advantage of hearing the history of a patient just as he might elicit it were the latter consulting him at his own office.

This patient says she has been suffering for two years, and yet she seems to look very well. Still, we have good reason to suspect that she is not perfectly healthy from the fact that she has not been pregnant for twelve years, although continuing all this time to live with her husband, and being desirous of bearing children, as she informs me. Well, two years ago

she began to suffer a great deal with backache, and although she is a healthy looking German woman, with an apparently fine physique, to feel very faint after exertion. Several times she fainted quite away, and this is certainly remarkable in a woman of her build. In addition she complains of dysmenorrhœa and irregularity in regard to the time of the catamenia; and these are all the symptoms that at present require our attention.

Of course, with such a history, I proposed a physical exploration. In some localities, and even among medical men, who ought to know better, there exists, I am aware, a great prejudice against making vaginal examinations; yet there are, of course, in every community a vast number of cases, such as the present one, which cannot be *honestly* prescribed for without such an examination being made. This is what I found on making one here. The uterus was almost in its normal position, though slightly anteverted; but alongside of it, on the right, was what I now show you upon the blackboard, an apparently globular mass as large as a baby's head. Again, on the left side of the uterus was a mass about the size of a walnut.

Now see what a flood of light has already been thrown upon the case by the examination. The physician, having discovered such a state of affairs as this, would scarcely think it worth while to continue to order plasters for her back, for instance, or to go on prescribing remedies for her dysmenorrhœa. In such a case you may not be able to relieve the patient after you have made the examination; but you will, at all events, have performed your duty in making it, and you may save her from a prolonged course of useless medication.

This patient, I may say, was sent to me by her attending physician ten days ago, but as she had at that time just finished menstruating, the large mass was so soft that I could not form a definite opinion as to whether the tumour was an ovarian cyst or a fibroid. So I requested her to return to-day, in order that we might decide upon the character of both the masses by the side of the uterus. To-day the larger of the two is very different to the touch from what it was immediately after menstruation. Then, as I said, it was very soft and succulent; but now it feels so hard as to exclude the idea of its containing fluid, and there can be no doubt that it is a fibroid. At present it has either become detached from the uterus, or else it is connected with it by a long pedicle.

The other mass, on the left side of the uterus, is of an entirely different character. It is exquisitely sensitive to the touch, and I have little doubt that it is an enlarged and prolapsed ovary. Like the large fibroid, however, it too was affected by the congestion of the parts incidental to menstruation, and when I made the examination before, it was both larger and even more sensitive than at present. The diagnosis is, then, chronic ovaritis on the left side, and large uterine fibroid on the right.

I do not know how long this trouble has been going on. The woman herself says, that, while she has been really suffering for the last two years, her symptoms commenced originally long before that; though she does not think there was anything whatever the matter with her more than seven years ago. It is my opinion, however, that this fibroid is the cause of her sterility, and this has now existed for twelve years, or ever since two years after her marriage. Uterine fibroids, as you know, are one of the most frequent causes of sterility that we meet with. But it is only within the last two years that she has suffered from any very definite symptoms.

Now what is the prognosis? Can we succeed in curing this case? In

all honesty I answer, "No." But if you ask me if I think this trouble will get well, the answer will be different; for when the menopause arrives, such fibroids sometimes undergo calcareous degeneration, and sometimes simple atrophy. But we also know that before the change of life occurs, there are certain remedies which seem to have the effect of diminishing the size of fibroids in a considerable proportion of cases. There are other cases, however, in which these seem to exert no beneficial influence whatever.

There is another question, however, which comes up: Can this growth be removed by operation? Just a month ago, as some of you will remember, I performed laparotomy for the removal of a fibroid of very much the same size as this one. Nor did I hesitate one moment about performing it, although it seemed extremely doubtful whether the patient would recover after it. But, fortunately, she did; and I think I would have been really culpable to refuse to operate in such a case as that was, because the patient would certainly soon have succumbed if active interference had not been practised. In the present instance, however, the circumstances are entirely different, and I should not think for a moment of operating.

The woman is now thirty-six, and in from six to ten years the menopause will occur; when there will undoubtedly take place a certain amount of atrophy in this large mass.

But in the other case there was the most marked abdominal dropsy, for which the patient had been tapped seven times; and during these different operations no less than two hundred and ten pounds of fluid were actually removed from her. From being a person of ordinary size, she had become so reduced that her weight was only ninety pounds.

The lesson that we learn from this case is, then, that simply because a uterine fibroid exists, there is no reason why it should be cut out. Why, if this were the case, about half the women in New York would have to be operated upon, and laparotomy would be performed for the removal of an insignificant fibroid no larger than a hen's egg. The fact that a patient may desire the operation herself, affords no argument in favour of it; nor should it influence us in such a matter, because it is one of the easiest things in the world to induce a woman to have such an operation performed, if it is only presented to her in a sufficiently favourable light.

As far as the present fibroid is concerned, I should suppose that this patient might live to be seventy or eighty years of age. It seems to me probable that it will increase but very little, if at all, in the future; although it is, of course, possible that it may continue to grow larger and larger during the next four or five years, and that after a time it may give rise to abdominal dropsy. When that period comes, it will be time enough to adopt operative interference; and the patient's fate will not be sealed, because we will then have the resources of surgery to fall back upon.

The proper course to pursue in this case at present is to remove the patient, if possible, from any position in which she is obliged to do hard work, either physical or mental, and in the next place to put her upon a very generous diet, including a great deal of animal food; in addition giving her such remedies, like iron, the hypophosphates, and the bitter tonics, as will build up her strength and general health. In other words, we are to do everything for her that will tend to bring her up to a point of physical vigor such as will render it impossible for this abnormal growth to increase any further in size. The very fact that a woman has one or

more fibroids is an evidence that she does not enjoy perfect health. This is also pre-eminently the case in regard to ovarian tumours; and these I may say are almost invariably met with among the lower walks of life, where the women live poorly and are obliged to work very hard. Although I have had a large experience with these cysts, I have never had but ten cases under my care in which the patients belonged to the highest classes of society. I am constantly meeting them among the poorer classes, and I have noticed particularly that there is always a depreciation of the patient's health before the tumour makes its appearance at all. This circumstance is not so markedly characteristic of fibroids; but, in general, the same rule applies to these also.

But though it would be entirely unjustifiable to operate in such a case as this, there are other methods of procedure which we may attempt for the relief of the trouble. Most of you are, no doubt, familiar with the extraordinary results which Hildebrandt has met with in the treatment of fibroids by means of the hypodermic injection of ergotin; the latter being merely a refined extract of the drug, and not its true alkaloid principle. In six months, he reports, he has repeatedly reduced a fibroid as large as a man's head to the size of a hen's egg. While I see no reason to doubt the statements of such a distinguished authority as Hildebrandt, however, I must say that no one else seems to have been equally successful in this treatment. This may be, however, because Hildebrandt has devoted more care to the systematic use of this drug than other investigators. In a certain number of cases I believe that the method is one of the highest service; and in the present instance I should recommend that about three times a week her attending physician should inject ten drops of Squibb's fluid extract of ergot somewhere in the cellular tissue of this woman's body. Sometimes ergot used in this way has the effect of producing the most violent uterine contractions; but this is to be avoided, and if it is found to occur, the amount of ergot injected should be diminished. What we really expect from the ergot is such an effect in causing contraction of the arteries that the supply of blood furnished to the tumour will be to some extent cut off, and its nutrition thus more or less impaired. When it is impossible to employ the ergot hypodermically, as is sometimes the case, we may use it either by the mouth or rectum; for although these methods are not so beneficial as the first, they are often of very considerable service. It is always unfortunate for such a patient, as I have previously had occasion to remark, to find out that she has a *tumour;* for women almost invariably associate the most terrible consequences with this word, and the moral effect of the discovery, in spite of the assurances of the physician, is apt to be quite bad sometimes.

CASE III.—*Large Uterine Fibroid.*

We have to-day another patient suffering from fibroid, and I have therefore thought it advisable for her to come in immediately after the one who has just left us.

This woman, whose name is Johannah M——, is thirty-four years of age, and I may remark, in connection with this point, that, although we are liable to meet with uterine fibroids at any period of life, as a general rule they do not make their appearance until the patient is over thirty years old. This patient has been married some years, but has never been pregnant. As to the cause of her sterility, perhaps we shall find out something explanatory presently.

How long have you been complaining?

"Ever since I was married."

In what way have you suffered?

"I do not suffer very much; but when I lie on my back I have considerable pain, and I can feel something like a stone in my stomach."

What else?

"I have a great deal of pain about a week before my monthly sickness comes on."

What else?

"I did have trouble in passing my water; but that is better now."

Is there anything else that you complain of?

"That is all."

Are you able to attend to your ordinary business?

"Yes."

Would you have liked to have a family?

"Yes."

Let me now describe to you, and roughly sketch upon the black-board, what the physical examination revealed here. In the first place, I found the cervix about in its normal position, and it was as small as we ordinarily find it in nulliparous women. Above it, however, there was a hard mass, as large as a man's head, which pressed in a marked manner upon the bladder in part and the rectum behind. Yet when I ask the patient how she suffers, she replies that she does not suffer much at all; for in her case almost the only symptom that usually seems worthy of attention is the sterility. It is possible that this mass alluded to may have existed for more than seven years; but we can hardly form an opinion in regard to that now, and even if it did, it was no doubt so small as to escape notice entirely.

It is a point of great interest to find out where the uterus lies in this case, and what is its relation to the tumour spoken of; and this is revealed by the introduction of the sound. In this way we ascertain that it is in the anterior portion of the mass, as is usually the case where there is a large fibroid of this character. The fibroid has no doubt originated in the posterior wall of the uterus, and has gone on increasing in size until it has involved the whole of the latter, and caused marked anteflexion.

As to the prognosis here it is exactly the same as in the other case. It is impossible to make a positive one, as we can in certain instances, but where the prognosis is certain, it is almost invariably bad, as in well-marked carcinoma, for instance. This fibroid presents some worse features than the one in the last case, and it is undoubtedly the cause of the woman's sterility. Pregnancy, however, would be the worst possible thing that could happen to this patient; because if she went to full term, the chances are that uncontrollable hemorrhage would occur after the delivery of the child.

What treatment shall we adopt in this case? The same remarks in regard to operative interference which were made concerning the last patient are equally applicable here. As to the irritability of the bladder which she has mentioned as having formerly troubled her, if it should return, I think she will find considerable relief from the wearing of a well-constructed abdominal bandage, which will to a great extent remove the pressure of the mass upon that viscus. The patient, I learn, is already under the care of a good physician, who has for some time been employing the method of treatment for the fibroid which I should have recommended

to him, viz., the hypodermic injection of ergot. She states that for about an hour after the injection she has a great deal of pain and "swells up." These phenomena, I have no doubt, are due to the contractions of the uterus, and also probably of the whole mass of the fibroid, set up by the action of the ergot; and this I have found to be a very frequent result of such hypodermic injections, having noticed it in quite a large number of instances. The pain, she says, does not pass off entirely until after about three hours. Altogether, she considered herself better than she was before the treatment was commenced, and the principal improvement she has noticed is the ease which she now gets at night; whereas she formerly suffered a great deal of pain when she lay down. She loses a considerable amount of blood at the time of her menstrual periods; but the flow does not seem sufficiently profuse to cause any anxiety. In addition to the treatment by ergot, she is also taking appropriate tonics. In regard to the hypodermic use of ergot, I may say in dismissing the patient that it sometimes causes such violent contractions of the uterus that the patient suffers fearful agony; and hence in such cases it is necessary to administer the remedy in some other manner.

CASE IV.—*Retroflexion of the Uterus, with Sterility.*

Our last patient to-day is Emma H——, a native of the United States, and thirty-four years of age. She has been married for fourteen years, but has never been pregnant.

How long have you been sick?

"Well, I have been really sick for six years."

Were you perfectly healthy at the time of your marriage?

"Yes."

What do you complain of?

"Pain in the side and back."

What else?

"I have a bad head. That is, I suffer from pain and dizziness in it, and sometimes feel very faint."

What else?

"I have been unwell for all the last month."

(I wish to pause here for a moment, gentlemen, to impress upon you the importance of always asking "What else" in the examination of patients until you come into possession of *all* the rational symptoms; for, otherwise some very important point may be entirely omitted which might have been of the highest service to you in your investigation of the case. When, some years ago, I conducted a "Diagnosis Class" at Bellevue Hospital, I found it the hardest thing imaginable to get the students to keep on asking this little question, "What else;" for as soon as they received an answer which they thought gave them a clue to the character of the case under examination, they at once started off on a special series of inquiries in regard to the point, and so were often completely misled. Instead of this, the correct way, as I have intimated, is in the first place to get the patient's story complete. But to resume our inquiries here.)

How about your monthly sickness ordinarily?

"I have always been perfectly regular up to the past month."

And since your last period you have been losing blood all the time up to the present.

"Yes."

Do you usually have much pain at the time of your monthly sickness?

"Yes, a great deal."

Do you have the whites?

"Yes."

I suppose you have never done anything to avoid pregnancy.

"No, sir."

Here, you see, is a perfectly healthy woman, who has been married for fourteen years without ever becoming pregnant; and hence we should be inclined to infer that she is not so entirely healthy as she looks. The two principal symptoms in her case we find to be sterility and dysmenorrhœa for the menorrhagia of which she complains is only of very recent origin. In looking back over the four interesting cases which we have seen to-day, you will notice that dysmenorrhœa was a prominent feature in all of them; and yet it is due to an entirely different cause in nearly every instance. In two of the women, you will remember, its cause was uterine fibroids. Twenty years ago, when the study of gynæcology was as yet almost in its infancy, dysmenorrhœa was considered a disease. Now we know that it is just as much a symptom as cough is.

The other symptoms of which this patient complained were, leucorrhœa and constant discomfort in one side; so that the necessity for resorting to a physical examination here was obvious. On employing conjoined manipulation, I at once discovered that the body of the uterus was not to be found in its ordinary position, and that there was some sort of a tumour or mass lying back of the cervix. When the posture of the patient was changed, and the probe was introduced, the latter was proved to be the body of the uterus in a state of marked retroflexion; as seemed highly probable from the condition of affairs found before the probe was resorted to. Retroflexion is, then, the diagnosis here, because it offers a satisfactory explanation of all the symptoms met with in the case.

In the first place, the sterility is undoubtedly due to it, because such a malposition of the uterus must almost necessarily prevent seminal fluid from passing up into its cavity. But while the entrance of seminal fluid is prevented, the retroflexion does not cut off the egress of menstrual blood; though it interferes with it to such an extent that much suffering is thereby caused to the patient. The menstrual blood must get out of the uterine cavity in some way; and if it cannot do so by means of the natural exit, it will force a passage through the Fallopian tubes into the peritoneum (as has been known to occur in certain rare instances), and form a "hæmatocele by regurgitation," as it is called. As to the leucorrhœa, that is easily explained by the constant state of congestion, in which the uterus is kept by its malposition; and the pain about the pelvis of which the patient complains is due to the tension upon the uterine ligaments and the direct presence of the retroflexed organ, which also gives rise to obstinate constipation of the bowels.

This is undoubtedly the most curable case that has been before us. I should not venture to say to either of the other patients that I could cure her; but here I do not hesitate to assert that the woman can positively be cured without any difficulty. I am willing to make this assertion from the fact that the uterus can readily be restored to its normal position. When the organ has been replaced, therefore, it only remains to maintain it in position by means of an appropriate pessary; although I think the probability of conception would be somewhat increased if the os internum were dilated from time to time with curved sounds. Still, it is best not to be too confident in regard to this point; as sterility is always a very uncertain thing to treat, whatever may be its cause.

# CASE OF FEMORO-POPLITEAL ANEURISM, WHICH HAD BEEN LAID OPEN BY MISTAKE, AND IN WHICH THE OLD OPERATION (THAT OF ANTYLLUS) WAS PERFORMED.

*A Clinical Lecture Delivered at St. George's Hospital.*

By T. HOLMES, F.R.C.S.,
Surgeon to the Hospital.

You lately had the opportunity of seeing a case which is so rare in its occurrence, and which conveyed so many important surgical lesions, that I must recall your attention to it.

The notes of the case may be thus summarized. The patient was a young man aged 29, a labourer, in fairly good health. He had had syphilis about six years ago, but no secondary symptoms. His habits were, as far as is known, moderately temperate. A year ago he fell from a wall about eight feet, and soon afterwards noticed a tumour in the calf of the right leg. No pulsation was ever noticed in this swelling. He was treated as an out-patient at St. Mary's Hospital for three weeks, after which he says that the swelling had quite gone. Five or six weeks ago, after a good deal of walking, he was loading a cart with coals, when he suddenly felt a sharp shooting pain down the right thigh, with a sensation as if something had given way about its middle; and half an hour afterwards, he noticed in this situation a swelling as large as a hen's egg. He went to bed, and remained in bed for a fortnight, after which he went to a neighbouring infirmary. During this time, the tumour steadily increased in size, but did not pulsate as far as he noticed. On his admission into the infirmary, careful examination was made in order to detect pulsation or *bruit*, but neither could be found. The tumour was twice explored with a grooved needle, and only dark-coloured blood was evacuated. On the day of his admission into this hospital (March 6, 1879), the tumour was punctured with a knife by the assistant of the surgeon to the infirmary. A jet of arterial blood issued from the puncture. The assistant plugged the wound with his finger, and sent for the surgeon, who enlarged the incision to the extent of about two inches. This was followed by a very formidable gush of blood. The wound was plugged with lint steeped in perchloride of iron, a tourniquet was applied, and the man sent to the hospital. When he arrived, the limb was much swollen, cold, and blue, and the patient much exhausted by hemorrhage.

With this history before me, my impression was that the tumour would probably be found to be of a cancerous nature, with a large blood cyst. It was clearly either this or an aneurism. In any case, the first thing to be done was to freely lay open the cavity. Accordingly, Esmarch's bandage was applied, and an incision about eight inches long was made, when the cavity was seen to be filled with the laminated clots externally and soft clots in the centre, which are characteristic of aneurism; and when all these had been removed, it was seen that there was no solid tumour, nor was it very clear whether the membrane which lined the cavity was a perfect sac or not. I determined to tie the vessel in the sac, and attempt to save the limb. It was by no means easy to find the opening of the artery; but at length what appeared to be the lower part of the sac was reached, situated quite in the popliteal space, and on tracing this round to its cæcal extremity, it was found to overlap a somewhat indistinct orifice about the size of a No. 10 catheter; and through this opening a probe passed for an unlimited extent downwards. This was accordingly taken to be the lower or distal opening of the vessel, and a silk ligature was tied around it. No proximal open-

ing could be found, however, in this situation; but on trying to trace the continuity of the vessel upwards, the great sciatic nerve was seen exposed for about three inches. Afterwards, the artery was looked for at the upper part of the tumour, and here an opening was found, from which the probe passed easily in both directions—upwards and downwards—and it was quite easy to dissect the vessel out from the neighbouring tissues. A ligature was passed round the artery both above and below the orifice, and then the bandage was taken off. No bleeding followed, except from a few insignificant vessels in the sac. These were tied, and the cavity was filled with carbolized lint, the wound being left open. When the bandage was removed, warmth began to return to the limb, which was then enveloped in cotton-wool, and the patient put to bed. The operation was a protracted one, lasting about an hour. His circulation was occasionally so low that it was necessary to suspend the anæsthetic.

I have been thus particular in describing the operation, because it is one rarely seen; but I need not detain you very long with the future progress of the case, especially as you have access to the copious notes and accurate drawings which my clinical clerk, Mr. Dale, made of it. The warmth and sensation of the foot at first seemed to return, but soon the whole dorsum of the foot began to slough, and the walls of the large incision also sloughed to a very great extent. When this latter slough cleared away, all the ligatures dropped off except the one tied on the proximal end of the artery, and the pulsation in the vessel could be seen down to this ligature on the eighth day after the operation. This ligature held on till the eighteenth day, when it came away without any hemorrhage. Meanwhile, the sloughing on the leg and foot extended and deepened; but the man remained free from any alarming general symptoms, and his complexion gradually regained a more natural hue. Amputation was delayed till April 3d, at which time all the soft parts on the dorsum of the foot had come away, exposing the tarsal bones, the joints between which were widely open, but the skin remained on the end of the toes. The gangrene of the skin had not spread beyond the level of the middle of the leg, so that it was hoped that a stump could be formed below the knee; but on cutting into the parts there to form the anterior flap, the muscles were found so rotten and so infiltrated with foul matter, that this idea was given up, and I amputated through the knee-joint, leaving the patella, as I believed that the parts forming the stump (which appeared perfectly healthy) would derive sufficient nourishment from the branches of the superficial femoral and profunda arteries; but in this I was disappointed. All the parts forming the anterior flap sloughed *en masse;* the end of the femur was freely exposed, with its cartilages; and so the unfortunate patient was condemned to a third operation. After the amputation, he suffered for a time from considerable shock; but this gradually subsided, and he seemed doing well; when, on May 15th, he was seized with a sharp attack of hæmaturia, for which there was no apparent cause. No antiseptic dressings were used during the whole case, and only occasionally a little weak carbolic lotion to syringe the sloughy cavity. The hæmaturia, however, passed off as it had come, though occasional sickness seemed for some days to testify to some kidney-irritation. At length, on June 6th, he seemed well enough to bear the removal of the exposed bone. Accordingly, I removed the patella and about four inches of the lower part of the femur, by simply pushing them through the remains of the great incision at the inner part of the thigh. The parts consolidated slowly round the end of the femur; and he left the hospital July 16th. He was seen again Aug. 9th, with the stump nearly healed.

Besides several minor points of interest, this case presents these two questions of primary importance: 1. How far was the original error of diagnosis avoidable, or, in other words, how far was the treatment pursued prior to the man's admission censurable? and 2, Was the course which I followed the right one?

As to the first point, the history is no doubt a remarkable one. In the seventh volume of the *St. George's Hospital Reports*, I wrote a paper on the Diagnosis of Aneurism, in which I collected and commented on most of the recorded cases of failure of diagnosis, in which I showed that almost all of them proceeded from negligence in examination, for that there was clear evidence that in the great majority the tumour had never been auscultated. At the same time, I adduced proof—and amongst others a striking case under my own care—to show that there have been cases of aneurism in which the most careful palpation and auscultation have revealed neither pulsation nor *bruit*. The practical caution which I deduced from these histories is the following. "Fluctuating tumours in the neighbourhood of large arteries should never be opened (unless they are visibly and unmistakably abscesses) without a previous careful examination of their history, their pulsation, their *bruit*, the effect on them of compression of the main trunk if possible; and finally, in case the doubt remains, an exploratory puncture. If this very common-sense precaution were observed, we should hear less of mistaken diagnosis in aneurism." You will possibly say that this is no novelty. I can only reply that the observance of this precaution would have saved many a life and many a surgical reputation. But in the case before us, all this had been done, except perhaps the experiment on the effect of pressure on the femoral above the tumour, which, however, ought never to be omitted in a doubtful case. The paper in question contains distinct proof of its high diagnostic value.[1] With this reservation, I do not see what more could have been done to establish the diagnosis. As to the treatment pursued at the infirmary, it might be perfectly correct up to a certain point. I am unable to estimate the indications for laying open the tumour, as I did not see the patient; but can easily understand that they might have been, and very probably were quite sufficient. It was evidently increasing, and seems to have been producing considerable obstruction to the venous circulation. But I cannot help thinking—and this I wish strongly to impress on you as a practical caution—that when it was judged necessary to make an incision into a tumour of doubtful nature situated

[1] The passage to which I chiefly refer is the following (*St. George's Hospital Reports*, vol. vii. page 181). The case is reported in Pirogoff's *Klinische Chirurgie*. "A man who had before suffered from a phlegmonous abscess in the thigh, and who was rheumatic, presented himself with a large inflamed swelling, indistinctly fluctuating, below the pectoral muscles and in the axilla. Not doubting that it was an abscess, Pirogoff had the scalpel in his hand to open it, when it occurred to him to auscultate the swelling. A low *bruit* revealed the true nature of the case. The symptoms are not very fully given; but the subclavian was ultimately tied. The patient died, and, on dissection, it was found that there was a small opening in the axillary artery, and the swelling consisted of blood extravasated under the pectoralis minor. From these cases, Pirogoff deduces the practical rule, never to open a deep-seated swelling, however plainly it may fluctuate, without previous auscultation, though he points out that *bruit* may exist in swellings which are not aneurismal (such as medullary cancer), and that in swellings which are in all probability aneurismal, the *bruit* may only be audible in parts. He thus sums up the whole subject: 'If I were asked what signs I hold most decisive of the existence of an aneurism which does not pulsate, I must confess that, if there be no *bruit* to be heard at any part of the tumour, I know of no other than these two—(1) collapse of the swelling, sometimes only to a slight extent, when the main artery is compressed between the heart and the tumour; and (2) if the pulsation of the artery can be felt upon the surface of the tumour, an unnatural extension of its impulse; for example, over twice the usual breadth of the vessel. But, in order to satisfy one's self of these two phenomena, it is, of course, necessary to examine the case repeatedly, and with the greatest attention. The diagnosis must not be founded on a single examination.'" Reference may also be made to a paper on aneurisms which do not pulsate, by Mr. Morrant Baker in the new (15th) volume of *St. Bartholomew's Hospital Reports*.

on the course of the main artery, and which had been previously twice explored with the result of showing that it contained blood, and, as far as we know, blood only, no incision should have been made without ample preparation for the ulterior proceedings, which are so likely to prove necessary; *i. e.*, amputation or the ligature of the artery. You saw the dangerous condition to which this man had been reduced, even after the short journey to this hospital, and you could easily imagine what would have been the consequence of a similar accident in a remote part of the country.

Next, as to the treatment which I pursued. To a certain extent, it was successful, as the man escaped with his life. But it is clear that if the thigh had been amputated at once, he would have been exposed to far less danger, and would, in all probability, have recovered some months sooner. Yet I am not on that account inclined to admit that I was wrong in performing the old operation, and so giving the patient a chance of preserving his leg. True it is, that the previous loss of blood and the œdema and coldness of the limb resulting from the prolonged compression rendered success improbable; but I think the attempt was a justifiable one. I am, however, free to admit, that, when I was forced to amputate, it would have been better to do so at the level of the upper ligature on the femoral artery. This would have saved the man some weeks of suffering, and given him probably a better stump.

I was hardly prepared for the difficulty which I experienced in finding the orifice of the artery in the sac. Even now, I must confess that I could not describe the state of parts with any confidence in the accuracy of my description, nor explain by what I found at the operation the singularities of the previous history of the case. That the upper ligatures were tied on the femoral artery was plain, both from the fact that it was really dissected out of the tissue of the sac at the time of the operation with something approaching distinctness, and also from the fact that after the separation of the sloughy walls of the cavity, the artery could be seen pulsating down to the ligature in this situation. But I confess to entertaining very great doubt whether the ligature which was first tied was really placed on the artery at all. I regard it as more probable that there was a rent in the sac through which the probe passed, and that what I tied around the probe was really a portion of the sac itself. If you consult the history of a case in which Mr. Syme laid open an aneurism in the iliac fossa, and believed himself to have tied the common, external, and internal iliac arteries (*Med. Chir. Trans.*, vol. xlv.), you will find that only the external iliac was really tied, one of the ligatures having probably been placed on a portion of the sac, and the other two on the external iliac above and below the opening. Nor is such an error at all surprising. It is not always an easy matter, even on the dead subject, to dissect out the artery from the interior of the aneurismal sac; and on the living, besides the necessity of some reasonable speed, even when the circulation is completely commanded, there are many sources of fallacy from which the dissection of the parts after removal from the body is free. As far as I could judge, however, I should conjecture that the femoral artery had given way, either completely or nearly so, somewhere near the opening in the adductor magnus, and the blood had poured into the cellular tissue of the limb (both in the thigh and popliteal space), making for itself an imperfect sac. At the upper part this sac seemed more firm and membranous; at the lower, it was less distinct and very probably wholly deficient at the part where the sciatic nerve was seen. If we adopt this view, it would get rid of one of the most difficult questions in the case, viz., why was there no pulsation? I have already, I think, insisted sufficiently (both in my paper above alluded to in the *St. George's Hospital Reports* and in other writings on Aneurism) on the indubitable fact that in almost all

cases of mere rupture of an artery without the formation of a sac, pulsation is absent. *Bruit*, too, appears sometimes to be absent, or at least not to be heard; for it is possible that in some of these cases at least the *bruit* has been low and only audible over a limited part of the tumour, and so has been missed (see Pirogoff's cases quoted in my paper). The former appearance of a non-pulsating tumour in the same situation which disappeared without any further symptoms is strange, and I cannot offer any probable explanation of it, unless it may be that a small rent then formed in the artery which closed spontaneously.

The rupture of an artery at a comparatively early age and without proof of general disease of the arterial system is, I dare say, rare; but I do not think it is so rare as is often believed. I have seen several cases which hardly admitted of any other explanation, and there is a preparation in our museum which bears so great an analogy to what I believe occurred in our patient, that I have thought it right to bring it into the lecture-room.[1] It shows the common carotid artery, in which, just below the bifurcation, there is a small opening leading into an enormous sac in the cellular tissue. The patient's age was the same as that of the subject of the present lecture—twenty-nine. The rupture occurred from no known cause, as he was at his ordinary work—that of kneading bread. The first symptom of it was a diffused swelling, which gradually became circumscribed and pulsated with a whirring sound, then became again diffused, and lost its pulsation. No operation was undertaken, and the patient died of suffocation. The history is rather incomplete, as the case occurred thirty-five years ago, before the present system of recording the histories of all cases along with their *post mortem* examinations was commenced. In this preparation, there are one or two small patches of atheroma near the orifice of the sac, but the chief part of the artery is healthy. You will at once see in how many respects the history of this case resembles that of our present patient. An artery gives way without known cause; a diffused swelling is the result, which pulsates when the sac is entire, but loses pulsation when the sac bursts. In the case which furnished the preparation, the old operation would have been, to say the least, very difficult, and might have proved impossible. But if it could have been completed without fatal hemorrhage, it would doubtless have saved the patient's life.

The lessons which the present case teaches may be summed up as follows: 1. It affords another example of the occasional existence of aneurisms (so-called) which do not present the ordinary symptoms of pulsation and easily recognized *bruit*. On this subject I have said as much as I usefully can in the paper to which I have already referred you. 2. It shows the difficulties and ambiguities which surround the old operations when performed in such cases. The cases usually present a large ill-defined and probably imperfect sac and a relatively very small arterial opening. 3. It shows the danger of meddling with tumours of this sort, unless the surgeon is prepared with all the appliances and assistance which he will require in order to undertake the formidable operation which may become necessary. 4. It shows how probable gangrene is in such cases, in spite of the evacuation of the contents of the tumour, and although only a small part of the artery may have been included between the ligatures. 5. It is an interesting example of the fact, so often noticed in gangrene occurring after ligature of a main trunk, that the visible destruction of skin and superficial parts does not always give a just idea of the extent to which the deep parts may be implicated, if not in absolute gangrene, at least in a low unhealthy degeneration; and therefore how much better it is in such cases to amputate through or near the level of the highest ligature.—*Brit. Med. Journ.*, Jan. 10, 1880.

[1] Series vi. No. 112, in the hospital museum.

# MONTHLY ABSTRACT.

## Anatomy and Physiology.

### *Coagulation of the Blood.*

Amongst the many researches that have been made in regard to the phenomena presented during the coagulation of the blood, comparatively few have been devoted to the precise determination of the time at which it occurs after removal from the vessels. Thackrah, in his well-known work, estimates it at from five to ten minutes for the horse, two to ten minutes for the ox, one minute to one minute and a half for the sheep, half a minute to one minute for the bird, one minute to three minutes for the fish, and two to four minutes for the frog. Nasse found the order of rapidity of coagulation in domestic animals to be the horse, goat, dog, ox, pig, sheep, and rabbit; but K. SCHOENLEIN has recently shown, in an article contained in the *Zeitschrift für Biologie*, that many circumstances render these statements unreliable. No distinction was drawn between the first appearance of coagulation and the solidification of the entire mass. No control experiments were made with other and similar quantities of the blood, and many precautions were neglected which experience has shown are requisite that the time of the coagulation should be rightly estimated. He has himself adopted a plan originally suggested by Vierordt, which consists in pricking the finger, and dipping into the drop of blood which escapes a capillary glass tube with thin walls, having a diameter of about one millimetre and a length of about half an inch. Into the other end of the tube, which is open at both ends, is inserted a white horsehair, thoroughly purified and cleansed by boiling in alcohol and ether. At first it will be found that on withdrawing the hair for a short distance no blood adheres to it, but as soon as coagulation has commenced small red coagulated portions of blood adhere to it, and can be removed with it from the tube. The coagula are very often thicker than the lumen of the tube, and thus show that they adhere very firmly to the hair—much more firmly, indeed, than to a glass tube. A small quantity of uncoagulated blood usually remains in the tube, which, even if it be churned with the hair, refuses to solidify. In some salamanders and tritons the process of coagulation takes place with so much energy that serum is almost immediately squeezed out of the newly formed clot. In most of the lower animals Schoenlein snipped a small piece of skin from the back after the hair had been removed by shaving; but in frogs the heart was excised and the blood obtained from it; and in fishes the gills were used as the source of supply; whilst in birds a feather was pulled out. By practice he was able to obtain the blood in the glass tube with the horsehair inserted in the course of from ten to fifteen seconds, and the time was always taken from the moment when the blood escaped from the bloodvessels.

The valuable part of Schoenlein's paper is a series of tables in which he notes the name of the animal from which the blood was taken, its sex, and weight, the temperature of the air at the time of the observation, and approximative numbers in seconds of the time of commencement and termination of the act of coagulation. We extract some of the results at which he has arrived. In the barbel (Barbus fluviatilis) in one instance coagulation began in the blood in 185 seconds after it had escaped, and was completed at the expiration of 205 seconds,

the whole period occupied being twenty seconds. In other instances the time of commencement was 125 and 95 seconds, and the duration of the act was 20 and 50 seconds. In the Triton cristatus the commencement of coagulation varied from 55 to 265 seconds after withdrawal, but in the former case it was completed in 20 seconds, and in the latter in 30 seconds. One of the slowest kinds of blood to coagulate was that of the Anguis fragilis, or blind worm, which did not begin to coagulate till after the lapse of 585 seconds, or nearly ten minutes, but even this was exceeded by a specimen from the Coluber natrix, or ordinary snake, which began to coagulate only after 1295 seconds, or more than twenty minutes, and was 410 seconds in becoming solid. This animal was, however, pregnant. A long list of birds is given, including the sparrow, starling, crow, woodpecker, pigeon, crane, swan, and others. In these the period of commencement of coagulation varied considerably, even for the same species—namely, from 75 to 495 seconds; in a large number the coagulation was completed in from 50 to 90 seconds, but in a few it lasted two minutes or more. In the mouse and rat the beginning varied from 85 to 245 seconds, and it was completed in from 50 to 180 seconds. Guinea-pigs presented similar variations. Ox blood varied from 37 to 555 seconds in beginning to coagulate, and from 90 to 210 seconds in completing coagulation. In man the commencement of coagulation varied from 145 seconds after withdrawal from the vessels, to 515 seconds, and the completion occurred at times varying from 3 to 10½ minutes. It will thus be seen that very remarkable differences are observable in the time of coagulation of the blood in different animals, the limits of variation differing frequently 10 to 20 per cent., for the commencement of coagulation, as well as for its termination, and in some instances very much more.—*Lancet*, Feb. 21, 1880.

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## Materia Medica and Therapeutics.

### *The Materia Medica of Antiseptic Surgery.*

In an interesting lecture published in the *American Practitioner* (March, 1880), Dr. JOHN CHIENE, Surgeon to the Edinburg Royal Infirmary, gives the following description of the armamentarium used by Mr. Lister in his practice of antiseptic surgery.

The best form of carbolic acid to employ is the absolute phenol of Messrs. Bowdler and Bickerdike Church, Lancashire. Its advantages are that it has no objectionable odour, is readily soluble, and does not irritate the operator's skin; while the more crude and impure forms met with are occasionally so disagreeable and harsh that some of the German surgeons anoint their hands with vaseline before beginning work, in order to obviate this inconvenience.

Among the various preparations of carbolic acid we may first take up the solutions. There are two watery solutions—strong and weak. The strong consists of one part of acid crystals in twenty parts of water. It is used for washing and purifying the skin and instruments; for soaking sponges, drainage-tubes, and horse-hair; and for the steam spray. The weak, which is half the strength of the strong—one part of the crystals in forty parts of water—is required for washing the sponges during an operation, for soaking the "deep dressing," and for dressing generally. These lotions should be filtered after making, and had better be kept in large, blue, glass-stoppered jars carefully labeled.

An alcoholic solution of the strength of one part of the acid in five of spirit of wine is employed for cleansing wounds seen a few hours after injury, and specially

for those cases in which dirt and foreign matter have obtained access to the tissues.

There are two oily solutions. The weak—one part of crystals in twenty of olive oil—is used for purifying and lubricating urethral bougies, sounds, and catheters immediately previous to their introduction; the strong—of one part in ten—for applying to exposed dead bone, in situations where we cannot at once remove it, but have to leave it for some time *in situ;* for example, in necrosis of the flat bones of the skull-cap. In such cases a piece of lint soaked in the oil is laid on the bare bone and covered with a piece of gutta-percha tissue.

Antiseptic gauze is prepared by charging unbleached muslin of open texture with the following mixture (New Formula, 1879): crystallized carbolic acid, one part; common resin, four parts; solid paraffin, four parts. This last prevents adhesiveness. Paraffin does not blend at all with carbolic acid in the cold, and therefore simply dilutes the moisture of carbolic acid and resin, without interfering in the least with the tenacity with which the resin holds the acid. The acid is only given off in sufficient quantity when the gauze is moist and at the temperature of the human body.

To charge the gauze the paraffin and resin are first melted together in a water bath, after which the acid is added, and all are stirred together. We have now to diffuse this equably through the cotton cloth; and this requires, first, that the cotton be at a higher temperature than the melting-point of the mixture; and secondly, that it be subjected to pressure after receiving it. The gauze is therefore heated in a trough, and as layer after layer is turned over the hot mixture is squirted on by means of a large metal syringe furnished with a series of perforations at the end. Finally, a large heated block is allowed to descend, which accurately fills the trough and subjects its contents to pressure. The quantity of fluid mixture employed should be somewhat less in weight than the amount of gauze.

The prepared gauze is used for the large superficial dressing; in loose pieces for padding and dressing irregular surfaces; for bandages; and also when wet, wrung out of one-to-forty aqueous solution, for the "deep dressing."

Mackintosh consists of thin cotton cloth having a layer of india-rubber waterproofing on one side. This should be evenly applied and continuous, so that the material is quite impervious. There must be no pin-holes in it.

Protective is made of oiled silk, coated on both sides with a thin layer of copal varnish, which renders the silk impervious to the carbolic lotion. Over this again a fine layer of carbolized dextrin is laid, which allows the one-to-forty lotion, into which the protective is dipped immediately before use, to wet, and so thoroughly purify the surface. The protective is neither aseptic nor yet antiseptic; hence the necessity of making it so before application. Its action is thus purely negative. It keeps the edges of the wound clean, moist, and free from the irritating action of the antiseptic employed, which, owing to the copal varnish, cannot penetrate to the wound; allows discharge to escape readily from under it into the dressing; does not adhere, and so is easily removed when necessary.

Carbolized catgut is thus prepared: To twenty parts of carbolic acid crystals add two parts of water, and to this again add one hundred parts of olive oil. Place this mixture in a flask, and in this put several skeins of catgut. These should be kept, by means of a few glass marbles or rods, above the level of the watery deposit which occurs. Seal the flask hermetically and set them aside in a cool place. The gut must not be used until five or six months after this, and the longer it has been prepared the better.

Carbolized silk is prepared by immersing a reel of silk in melted beeswax con-

taining about one-tenth part of carbolic acid. The silk is drawn through a dry cloth as it leaves the hot fluid, to remove the superfluous wax.

All these various requisites should be kept by themselves apart from all other dressings—the gauze in a tin box; the silk in a stoppered glass jar; sponges, drainage-tubes, and horse-hair in wide-mouthed jars of one-to-twenty lotion; and the gut in its oil.

The various forms of steam sprays employed are constructed on the principle of Adams's steam inhaler. The boiler should be strong, dome-shaped, and furnished with a safety-valve. It is filled by an aperture situated at the lower level of the dome, so that in filling this space is left clear for steam alone. It is a disadvantage whon the boiler is filled at the very summit; and in hospitals sufficient care is often not exercised, the steam dome is encroached upon, and a jet of boiling water is thus thrown out in place of spray. The steam-pipe, provided with a stop-cock and ball joint, passes forward from the top of the dome and ends in a fine point, through which the steam rushes with great force. United to the under surface of the steam point, at an angle of forty-two degrees, we have the carbolic point continuous with the upper extremity of the India-rubber tube which leads up from the reservoir of one-to-twenty lotion. As the steam rushes out over the carbolic point it creates a vacuum, and the lotion thus sucked up is driven off in a fine cloud of vapour which covers an area large enough for any ordinary operation, and which is quite respirable, not wetting, and effective at a distance of at least four feet.

On arriving at a patient's house we fill the spray with boiling water up to the base of the dome; never above this; and so we avoid the danger, as I have remarked, of having the upper point blocked by particles of dust carried along in the jet of water, which would ensue were the boiler overfilled. We light the lamp, noting that the wick is in good order and that there is a sufficiency of spirit. We judge that steam is up if it escapes with great force and if it has a distinctly blue colour, when we shut off all carbolic acid, which may readily be done by compressing the carbolic tube with the fingers, and so seeing steam alone. One has also the peculiar rushing sound, the smell and taste of the spray to guide him in ascertaining if all is in working order. A small filter formed of a piece of sponge, inserted into the lower extremity of the carbolic tube, and secured in position by means of a gauze cap, will prevent the lower point from getting choked with dirt, which, falling into the open jar of acid, may be sucked up, and so cause trouble. Should the spray cease working, we may unscrew the points and affix the reserve pair found in the hollow handle, the wound being meanwhile protected by a "guard" consisting of a rag or piece of gauze soaked in lotion. In this way operative procedure is not hindered, and the defaulting points may be seen to and cleaned out with a horsehair or fine silver wire at a more fitting time. This is an accident which hardly ever occurs in private practice.

The other antiseptics employed are—

A solution of chloride of zinc (forty grains to the ounce of distilled water), introduced by the late Campbell de Morgan. It is chiefly used to brush over the cut lips of incisions and wounds in regions which we cannot hope to keep aseptic, as in excision of the upper jaw or lateral lithotomy. We may leave our dressing of strips of lint soaked in this solution *in situ* for forty-eight hours, so potent is this salt; and in this way, thanks to its searching character and non-volatility, the pain and unrest of dressing is avoided, and a dangerous period, during which blood-poisoning from absorption might take place, is tided over. Considerable smarting and pain ensue after application, and this continues for a varying period, according to the temperament of the patient. The use of chloride of zinc for purifying ulcers will be referred to shortly.

Boracic or rather boric acid is used as lotion, lint, and ointment. It is non-volatile, very unirritating—in fact the least so of all antiseptics—but is not at all searching. It may prevent, it can hardly eradicate putrefaction. The lotion of one part of the crystals in thirty parts of water is coloured red with litmus, and thus at a glance we may distinguish it from other lotions. It is used for moistening the boric lint and for washing sores.

The lint is prepared by soaking ordinary surgeon's lint in a boiling saturated solution of boric acid, coloured red with litmus. It is allowed to cool, the lint is hung up to dry, and the remaining fluid poured off and used as boric lotion. The lint is of a pink hue, and glitters with the soft, flat micaceous crystals. In a similar manner we may charge bibulous paper or the paper lint introduced by Messrs. Wyeth, of Philadelphia. We moisten the boric lint with boric lotion before application, and this for the same reason as we also soak the deep dressing of gauze or the protective in carbolic lotion. The surface of the material may be covered with germs of all kinds, because the antiseptic is not acting. We destroy these organisms by our active lotion, and as the aseptic discharge finds its way afterward into the dressing it dissolves and sets free quite enough of the stored up agent to render itself also antiseptic.

Boric ointment may be prepared by rubbing up one part of finely levigated boric acid in five parts of vaseline. It acts as a sort of antiseptic protective, and is specially useful in the treatment of wounds in the face, where it allows the discharge to escape, keeps the wound sweet, and never adheres.

An emulsion of salicylic acid in one-to-forty carbolic lotion was introduced by Mr. Lister for the purpose of checking the chemical changes which may take place under dressings which have been left unchanged for some time. These changes, due to a chemical action between the gauze and the discharges under it, the sweat, etc., give rise sometimes to a troublesome irritation and eruption, formerly dubbed *eczema carbolicum*. A very little salicylic cream smeared on the surface of the protective or deep dressing effectually disposes of this.

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*Iodide of Potassium and Calomel in Ocular Therapeutics.*

Dr. W. SCHLÆFKE directs attention (*Arch. f. Ophthal.*, xxv. 2) to the danger of dusting the eye with colomel while the patient is taking iodide of potassium internally. He shows (1) that the latter drug enters the circulation and penetrates to every part of the system with great rapidity; its presence in the various secretions, more particularly in the tears, may be demonstrated in a few minutes after administration. (2) That when iodide of potassium is being taken, even if only to the extent of seven or eight grains twice daily, it is constantly present in the tears in demonstrable quantity. (3) That calomel, though only very sparingly soluble in water, dissolves ten times more freely in a ¾ per cent. solution of common salt. (4) That calomel, when dusted on the conjunctiva, is dissolved to some extent, and unchanged, in the lachrymal secretion, and then acts as a chemical agent. (5) That when calomel comes into contact with tears containing iodide of potassium in solution, iodate and iodide of mercury are formed. (6) That both of these substances are soluble in the presence of iodide of potassium or common salt, and that when so dissolved they have a powerful caustic effect on the conjunctiva and excite violent inflammation therein. (7) That for the foregoing reasons the external use of calomel should be avoided so long as the tears show traces of the presence of iodide of potassium.—*Glasgow Med. Jour.*, March 1880, from *Centralblatt f. d. Med. Wiss.*, Jan. 21, 1880.

*Pyrogallic Acid as a Remedy in Skin Diseases.*

During the year 1879 experiments have been made at Vienna, Paris, Breslau, and elsewhere with ointments containing pyrogallic acid instead of chrysophanic acid, and with good results. Psoriasis is the disease in which it has been chiefly used, and the most convenient strength has been found to be that of one in ten. Most of those who have tried it—for instance, Professor HEBRA (*Wiener Med. Presse*, Nos. 43, 44, 1879) and Professors BESNIER and VIDAL of the St. Louis Hospital (*France Médical*, March 12 and July 5, 1879)—prefer the remedy to chrysophanic acid. They find that, though its action is slower, it has the advantage over the latter in exciting scarcely any inflammation in the part to which it is applied, and in staining it but slightly, the brown colour produced by it quickly disappearing. Its price is also said to be less than that of chrysophanic acid. Hebra finds that a patch of psoriasis treated with pyrogallic acid ointment takes from one to three weeks to disappear. He has used it also in tinea tonsurans, in lupus vulgaris, and in syphilitic papular eruptions. The lupus nodules are destroyed in about three days, probably, as we shall see directly, owing to the affinity of the acid for oxygen.

Hebra has never yet seen any poisonous symptoms follow its application to the skin, although in his cases it could always be detected in the urine. A case has, however, been lately reported by Dr. A. NEISSER (in the first number of Leyden and Frerichs' *Zeitschrift für Klinische Medicin*), where a patient died from pyrogallic acid poisoning in the skin clinic at Breslau. He was a robust man suffering from psoriasis of the whole body, and was certainly not experimented on with much caution. One half of his body was painted with chrysophanic acid, the other with 10 per cent. pyrogallic acid ointment. Six hours after, he had a severe rigor attended with vomiting and collapse. The symptoms then subsided, but returned in about forty hours, and from that time he never rallied, but became comatose and died on the fourth day after the painting. The peculiarity of his urine, of which he only passed about one litre and a half during his illness, attracted attention. It was dark brown, and deposited a thick sediment, which consisted of a very abundant blackish-brown substance, partly amorphous, and partly in the form of drops or casts, but it contained no blood-cells. The spectrum showed the characteristic bands of hæmoglobin, and similar *débris* to that in the urine was found in the blood itself and in the renal tubules. There could be no doubt, in fact, that it consisted of disintegrated red corpuscles.

Dr. Neisser now experimented on rabbits both with chrysophanic acid and pyrogallic acid, and, after proving the harmlessness of the former, discovered that the latter, in moderately poisonous doses, causes rigors, dyspnœa, tremor of the limbs, general weakness, and hæmoglobinuria, with the same changes in blood and urine as were found in his patient. The animals may recover if the dose is not excessive, but with a large dose they die with all the above-named symptoms plus the alterations of the blood corpuscles, but there is no hæmoglobinuria. With a very large dose (two grammes of acid per kilogramme of body weight), to which they generally succumb in about two hours, there is neither hæmoglobinuria, nor can disintegrated blood-cells be detected in the blood; the latter, however, is either blackish or chocolate-coloured, and sometimes of the consistence of jelly.

Dr. Neisser explains the poisonous action of the acid on man and animals by its avidity for oxygen in the presence of alkalies, and the consequent destruction of the red blood corpuscles, which are the carriers of oxygen. The hæmoglobin thus set free is dissolved in the plasma of the blood, and the corpuscular *débris* remains suspended in it, and is in part excreted by the kidneys. He considers that the infarction of the latter does not directly cause death by retention of the

urinary constituents in the blood, but by delaying the excretion of the pyrogallic acid from the system, and so prolonging the period of its deoxidizing action. It seems to us, however, that where death follows maximal doses it must be directly due to so large a destruction of red corpuscles that those which still remain intact are unable to satisfy the need of the organism for oxygen, and there is not even time for the appearance of hæmoglobinuria. Dr. Neisser considers that the external use of pyrogallic acid should be restricted to the head and face, chrysophanic acid being still retained for the skin of the trunk and limbs. Theoretically, transfusion with simultaneous venesection would be the rational treatment of the poisoning, and active measures should be taken as early as possible to promote diuresis, and clear the renal tubules of *débris*. After all, however, the number of individuals liable to be poisoned by pyrogallic acid may be limited, just as the number of those who die from chloroform is; but there is no doubt it is a drug which cannot be safely played with.—*Med. Times and Gazette*, Jan. 31, 1880.

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## Medicine.

### *Cases of Rheumatic Fever treated with Salicylate of Soda, and terminating Fatally.*

Dr. GOODHART, at a recent meeting of the Clinical Society of London, read notes of a case of acute rheumatism treated by salicylic acid, and terminating fatally. The patient was a servant girl, aged seventeen, with a history of gout and rheumatism on the father's side; but she had always been healthy, except that she suffered from nasal polypi, and had been three times operated on for that affection, the last time fourteen days before her fatal illness. She had had scarlatina long ago. After the last operation for the removal of the nasal polypus, she felt faint and weak, but continued at work for a week, when one morning she complained of pain and stiffness of joints, and had headache, anorexia, but no rigor or vomiting. She was treated for rheumatic fever and got better, but relapsed in two days and came to the hospital. She was a dark-complexioned girl with flushed face and bright eyes; tongue was red at edges, with a white fur on the dorsum, and rather dry. The ankles, knees, wrists, and elbows were much swollen, the skin pinkish. The phalangeal joints were also swollen. Precordial dulness increased; apex beat normal; pericardial subsultus at base; temperature 103°; pulse 120. Bicarbonate of potash was administered in half-drachm doses, in acetate of ammonia, every three hours; with five grains of Dover's powder night and morning; and milk diet. Next day the area of precordial dulness reached to second intercostal space, across from sternum to nipple, and downwards to the sixth cartilage. The impulse was in its normal position; the action regular; the sounds very distant and thick at the base, but there was no bruit. The lungs were normal; urine of specific gravity 1021, not albuminous. She appeared to have a good deal of precordial distress; was sweating freely; and the joints were still painful. The temperature had gradually fallen from 103° to 100°. The alkaline mixture was continued, but at 6 P. M., as she still seemed in much pain and the temperature was rising again, though very slightly (100.4°), she was ordered fifteen grains of salicylic acid every three hours in its place. She was seen by Dr. Goodhart at 9 P. M., and though still in a good deal of pain and restless, the pulse was still at 120, and there were no particularly serious symptoms. But after this the pulse became much accelerated, reaching 160, and the patient died quietly and suddenly at 3.45 A. M. She had

taken four doses of salicylic acid, sixty grains in all, the last dose three-quarters of an hour before death. She passed a restless moaning night, but did not seem unusually ill, nor was anything unusual noticed till her death.

On inspection, the heart weighed nine ounces and a half; there was much ecchymosis of the pericardium, and patches of recent lymph in several places, but no fluid. The muscular tissue of the ventricle showed one or two points of fatty degeneration, but was otherwise healthy, and the ventricle was not dilated. The valves were practically healthy, beyond a grain or two of lymph on the mitral. The right ventricle was distended by clots of post-mortem formation continuous with similar clots in the pulmonary artery. The small intestine was much ecchymosed and congested, and the joints all contained abundant yellow synovia with coagula of yellow lymph, especially the knees and hips. All the bones were normal, special search being directed to the sphenoidal, ethmoidal, and frontal sinuses in the region of the recent operation.

The first appearance of the patient, her dry skin, and her restlessness argued badly for her recovery, and suggested the possible event of hyperpyrexia, but as the temperature progressively fell, and rheumatic sweating became established, these apprehensions subsided, and the case assumed the characters of an ordinary attack of acute rheumatism of by no means severe type. Sudden death was therefore an unexpected event, and the question was, Is the result to be attributed in any degree to the salicylic acid treatment? Dr. Goodhart thought this possible, but did not wish to press that opinion unduly, in the face of facts which rendered the question a very open one. 1. An early pericarditis was present, and this with the fatty change, though slight, might have sufficed to determine the cessation of the heart's function. 2. It might be suggested as possible that, in the four hours' interval between being seen by any skilled assistant and her death, the temperature might have risen rapidly and death occurred from hyperpyrexia. 3. The clot in the pulmonary artery might have been embolic. But as to the first view, the change in the heart-wall, and especially the absence of dilatation, did not seem sufficient to explain sudden death; as to the second, the température chart was one of almost progressive fall; and, thirdly, the character of the clot and its continuity with that in the ventricle justified the rejection of any hypothesis of pulmonary embolism.

Dr. Goodhart then discussed the possibility of death being due to the rheumatic *materies morbi*, or to the sixty grains of salicylic acid, which had been administered. Acute rheumatism was a very treacherous disease, especially in young adults; but before the introduction of the salicylic treatment his post-mortem experience led him to the conclusion that death was associated in some cases with hyperpyrexia, without obvious tissue change; in others with pleuro-pneumonia, pulmonary œdema, and *severe* cardiac complications; occasionally, and comparatively rarely, with acute dilatation of the heart. Since the introduction of salicylic acid, he had seen three cases where death was sudden, unexpected by the physician in charge, and in which the inspection did not discover any changes in the viscera sufficient to cause death. Was it a coincidence that these cases had occurred since? All the three were girls or young women, and in all it was a first attack. Coming, as this case did, the last of the three, determined him to bring it forward, as one, possibly, in which salicylic acid had acted prejudicially in a manner and with a rapidity hitherto unrecognized. This was the more necessary because the mode of death had no special symptoms attaching to it which pointed decidedly to the drug as a harmful agent; and under similar circumstances, supposing that death was caused by the drug, it is quite possible that the ill result might be attributed wrongly to other causes. Death in such cases must, if ascribed to salicylic acid, be attributed to sudden collapse and failure of

the heart's action, symptoms known to be produced by salicylic acid, and some such cases of sudden collapse have already been observed and recorded by others, though perhaps rather in association with a more sudden fall of temperature. Such cases have led to the recommendation of caution in the size of the dose of the drug in cases where there should be any sign of a failing heart, or towards the end of an attack. But in the case under consideration there was no such indication for caution. Moreover, caution was never overstepped; so that the result, if it be a result, could only be averted, if at all, by commencing with a very small dose and gradually increasing it. Dr. Goodhart thought that, in dealing with a drug which so quickly and so completely saturates the system, which, in some cases, and one could hardly foretell when, produces severe nervous symptoms, in others sudden collapse, this is always the wiser plan to adopt, and in many cases the smaller doses seemed to him to be all that are required for the relief of pain.

Dr. BUZZARD then read notes of a case characterized by symptoms of acute rheumatism, which terminated fatally on the tenth day. The patient was a previously healthy lady, aged forty-two, who got a chill coming out of the opera on July 26th, and four days afterwards was taken with shivering, "queerness" in her head, noises in her ears, and sleeplessness. When seen by Dr. Buzzard on Aug. 3d, in consultation with her own medical attendant, Mr. Airey, she lay in bed complaining of these symptoms—of deafness in the right ear and imperfect hearing in the left, with constant nausea and aversion from food. The pulse had been at times as low as 56, and there was much prostration. The temperature was normal, there was no paralysis, diplopia, or change in the fundus oculi; the heart-sounds and breathing were normal. She had never had otorrhœa, and the deafness had only been coincident with her illness. Restorative food and remedies were prescribed. On Aug. 5th a great change had taken place. The temperature was 103° F.; pulse 106; face flushed; left knee-joint swollen, very painful and powerless. There had been much sweating. Urine acid. Heart-sounds normal. She was ordered fifteen grains of salicylate of soda every three hours. On Aug. 7th, Dr. Buzzard found the temperature 99°, the pulse 108, the respiration 38, the heart-sounds normal; the state of the lungs behind could not be ascertained. The swelling of the left knee was considerably reduced, but the right knee was now greatly swollen and painful, and also one of the finger-joints. Careful inquiry and examination failed to discover any wound or sore. There was great prostration. Brandy in frequent doses was prescribed, and the salicylate ordered to be given at longer intervals. The patient sank next day. Dr. Buzzard remarked that he saw the patient upon three occasions. On the first the symptoms were entirely cerebral in character; on the second, although they pointed strongly in the direction of acute rheumatism, yet he could not help a strong feeling that the case was one of pyæmia, the cause of which, however, could not be ascertained. No doubt pyæmia did occasionally occur without any discoverable wound; he had known it happen in connection with a simple fracture, and in another case from an ulcer of intestine, found after death. In the present case all efforts to procure an autopsy were fruitless. He had not the slightest belief that the salicylate had been of injurious influence, as the patient's condition from the first was exceedingly grave. Not more than twelve doses were taken.

Dr. BRISTOWE called to mind two cases of sudden death in acute rheumatism. One was a case of a previously healthy young woman, who died in syncope, without any explanation; and post-mortem examination showed nothing beyond the articular inflammation. This was long before salicylic acid came into vogue. The other case occurred two or three years ago, and was also not treated by this drug. The subject was a young woman, with no very marked symptoms, freely

sweating, a high but not excessive temperature. She died suddenly at the end of a few days. There was a trace of pericarditis found, and also some partial fatty degeneration of the myocardium, which did not, however, show any obvious inflammatory change. The case related by Dr. Buzzard was probably one of pyæmia. Quite recently, at St. Thomas's Hospital, he (Dr. Bristowe) saw a girl thought to be suffering from acute rheumatism, with a hot skin; but she had had rigors; complained of pain in the course of the humerus. She was transferred to the care of a surgical colleague, who, suspecting some acute necrosis, excised the shoulder-joint, when his surmise was found to be correct. The case ended fatally, and, after death, pus was also found in the elbow-joint. There was suppurative (pyæmic) pericarditis, and the lungs were studded with infarcts.

Mr. H. Lee could not admit that either of the cases read were acute rheumatism. Surgeons were well acquainted with instances of septic poisoning simulating this disease, and following even the slightest injuries. Some of these cases presented an eruption on the skin like scarlet fever. In Dr. Goodhart's case the febrile attack ensued upon the removal of a polypus, and the post-mortem evidence favoured the view of the septic nature of the disease in the material alteration found to exist in the blood and the ecchymoses under the pericardium.

Dr. Green could not feel sure that the cause of death in Dr. Goodhart's case might not have been due to changes in the muscular tissue. One or two patches of fatty degeneration were found, and the contracted condition of the ventricle could well be explained if rigor mortis had not passed off; for there is no doubt that rigor mortis might exist in very degenerate hearts, so that an organ greatly diseased would yet appear fairly well contracted The sudden death pointed strongly to its cause being due to changes in the muscular wall.

Dr. Glover did not think the case had much bearing upon the question of the toxic effect of salicylic acid, and it would not be right to draw any unfavourable inference upon this drug from an isolated case. He thought Dr. Bristowe's suggestion as to Dr. Buzzard's case to be probably the correct one.

Dr. Maclagan said there was no question as to Dr. Goodhart's case being one of acute rheumatism, and he thought with Dr. Green that it would be well if more information had been afforded as to the state of the heart. As to the whole subject of the share taken in the ill result by salicylic acid, it must be remembered that it was now given very largely in many febrile and non-febrile diseases, but yet disastrous results were only recorded in connection with acute rheumatism. Dr. Bristowe's remarks pointed to the fact that, quite apart from salicylic acid, the rheumatic poison alone would in some cases lead to sudden death. Myocarditis, too, was a more frequent complication of acute rheumatism than was generally thought. There was no doubt that inflammation extended from the pericardium to the muscular substance; but such extension could not take place in endocarditis, which is always limited to the valves; and he thought that the inflammation might primarily involve the fibrous structures of the heart, as those of other parts in acute rheumatism. Thus it might extend from the fibrous rings at the orifices to the muscular substance. Senator's views as to the conversion of salicin into salicylic acid in the blood involved an error. Dr. Maclagan mentioned a case where the pulse increased in rapidity, and became weak under salicylate of soda, but improved when salicin was substituted for this.

Dr. Mahomed had seen Dr. Goodhart's case, which was undoubtedly one of acute rheumatism. He had been struck with the frequency with which the first sound of the heart failed, and even disappeared, during convalescence after a prolonged course of salicylate of soda in acute rheumatism. He was much struck by this in a case of typhoid fever, where the salicylate was given; the pulse-rate rose from 120 to 160 within thirty-six hours, and so feeble was it that death

seemed impending. The drug was discontinued forthwith, and in twenty-four hours the pulse fell to 120, and the case subsequently improved. He had not the least doubt that, had the patient had one more dose of the salicylate, he would have died. He added that he had never observed so marked a degree of failure of the first cardiac sound in fever, as in these cases treated by the salicylate.

Dr. BARLOW asked if the tympanic membrane had been examined in Dr. Buzzard's case. The first symptoms were shivering and pain in the ears, and he suggested that possibly the case was one of suppurative internal otitis (without rupture of the membrane) giving rise to pyæmia. The fact that the effusion passed away from one joint and appeared in another did not tell against pyæmia; for he had seen this occur in two or three cases.

Dr. DOWSE had prescribed salicylate of soda largely, not only in rheumatism, but in other painful affections—*e. g.*, the pains of ataxy—always with relief to pain, but sometimes with alarming symptoms of depression. But he had found that salicylic acid alone (which was quite as valuable in painful affections) did not have such depressing effects. He doubted if Dr. Goodhart's case was one of rheumatic fever; and he referred to a case with high temperature and profuse sweating (not of an acid character), which, at first thought to be acute rheumatism, turned out to be due to phlebitis.

Dr. Goodhart, in reply, said there was no question at all that his case *was* acute rheumatism. The fact of the patient having had a nasal polypus removed so shortly before the fever set in made him search carefully for any evidence of blood-poisoning, and he still admitted the remote possibility of its being of such a nature. But he submitted that it was just these obscure cases of septic poisoning that came under the cognizance of physicians rather than of surgeons, as Mr. Lee had said. In reply to Dr. Green, he said that the muscular substance of the heart was carefully examined microscopically, but only with the result he had stated. Still, the heart might fail and its muscular structure not be diseased. He had looked through the literature of the subject as to the distinction between salicin, salicylic acid, and salicylate of soda, and confessed that it threw no light upon the matter. He agreed that, until cases of this kind were brought forward and discussed, it would be impossible to draw conclusions as to the part played by salicylic acid in the fatal result.

Dr. Buzzard, in reply, said that he was careful not to describe his case as one of rheumatic fever; but at the same time careful examination had failed to reveal any source of pyæmia. Against Dr. Barlow's view was the fact that the deafness was at first complete in one ear, and then the other ear became slightly deaf. The case showed rather cerebral exhaustion than acute inflammation. He did not attribute the fatal issue in the least degree to the salicylate of soda.—*Lancet*, Jan. 17, 1880.

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### *The Cause of Intermittent Fevers and the Nature of Malaria.*

Professor KLEBS, having gone to Rome last spring with the intention of investigating the cause of malarial fevers, with the assistance of a Professor of the Roman University, Dr. TOMMASI-CRUDELI, he was soon able, thanks to the kindness of several Roman land-proprietors, to obtain the necessary material for those researches, from the soil and air of the infected districts, partly of Rome itself, partly of its environs, so ill-famed for fevers—that is, from the Roman Campagna and the Pontine Marshes.

From the introduction to their pamphlet we see that the two investigators followed a very deliberate plan in the performance of their task. First, they furnish us with a very able description of the mode of propagation of the malarial

fevers. This naturally forms the basis of their investigations into the cause of the disease, the ideas about which have been so vague as to induce constant changes in the theories of its origin. A fresh theory takes the place of the previous one when recent facts and experience concerning the outbreak of the disease become known. If it be true that marshy districts are the favourite seats of intermittent fevers, it is not the less true that there are no fresh cases to be observed so long as a sufficiently high level of water separates the soil from the air. Further, it is no longer possible to consider malaria simply as a marsh miasm; for it is not found in every marshy district, whilst, on the contrary, it shows its existence in districts absolutely free from marshy ground—even where the soil has never been constantly wet. It is also impossible to state a distinct connection between the chemical and geological structure of the soil and the manifestation of malaria. Again, the accumulation of decaying organic material in large quantities is not sufficient alone to explain an outbreak of intermittent fever. All these conditions, which, it is true, have their part in the manifestation of the miasm, are to be considered only as adjuvant causes. It has been established beyond doubt, that in malarial districts three distinct factors are necessary for the production of intermittent fevers—(1) high temperature of the air; (2) constant moisture of the soil; and (3) free access of air to the moist layers of the ground. Several pathologists have been induced by such facts to deny altogether a specific cause of malaria. They numbered this disease amongst those caused by cold, because, according to all experience, the last-named condition takes a great part in the outbreak of the disease; but they completely forgot that in countries free from malaria, cold alone never causes intermittent fevers, and that, on the other hand, the malarial poison causes those fevers without any previous cold. Others formed the idea that malaria might find its origin in a parasitic organism, whose development must necessarily depend not only on favourable external conditions, but on the existence of a specific germ. Beyond the analogy of intermittent fever with other infectious diseases undoubtedly of parasitic origin, such as relapsing fever and splenic fever, one circumstance goes far to corroborate the parasitic theory—namely, the fact that malaria undergoes a very rapid increase, when the known favourable conditions for its development are no longer restrained by culture of the soil. Italy furnishes numerous illustrations of this point, because the malarial places have nowhere else been known with such exactitude from ancient down to recent dates; and because the difference between ancient and modern times as regards the prevalence of these diseases is far too great to be attributed simply to chemical and physical changes in the soil. Such an enormous increase is much more easily explained by assuming that a parasitic organism might have spread to such a large extent because the obstacles to its development had ceased to exist. The fact that the malarial poison is limited chiefly to the layer of air situated immediately above the surface of the soil, points to a corpuscular nature of the poison, because such particles can spread farther only by motion of the air, whilst a hypothetical gas would find no difficulty in diffusing itself through the upper layers of the atmosphere—even if we have no regard to the fact that no such gas has ever been detected by chemical analysis.

During the last twenty years many efforts have been made to find a specific malarial fungus—with more or less success. Whilst a certain number of scientists charged some forms of algæ, found partly in the marshy water of malarial districts, partly in the marshy soil, with the production of malaria, but failed to give real proofs for their assertions, others endeavoured by experiments on animals to attain the solution of the problem. Griffini injected dogs and rabbits with dew collected from rice- and marshy fields, and produced by such injections elevations

of the bodily temperature, but no distinct febrile attacks. Moreover, the spleen in those animals remained normal, and there was no augmentation of the organisms found in the dew to be observed in their blood. Similar injections made by Lanzi and Terrigi in the year 1870 gave no more distinct results, but in several of the animals which had perished after a smart attack of fever, the liver, the spleen, and also the blood of the portal vein, contained black pigment. Thus this method of investigation seemed not altogether quite unsuccessful. To establish a substantial theory of a parasitic origin of malarial diseases it is necessary to prove (1) the constant existence of a definite species of organism in the malaria-soil as well as in the air situated above such a soil; (2) that this same species of organisms is alone sufficient, without the concurrence of any other disease-generator, to produce true intermittent fever. To obtain the latter proof it is also indispensable to experiment on animals; and such an experiment would be the more conclusive, the better we should succeed in isolating an *active* organism from the marshy soil, and in introducing it in a suitable medium into the living animal. Further, it would be necessary to have the three following conditions realized, to enable us to recognize the disease produced in the animal by the introduction of the organisms as true intermittent fever—namely, (1) the existence of distinctly intermittent feverish attacks; (2) the enlargement of the spleen in the characteristic form—that is, a well-defined firmness of that organ, which is distinguishable from the enlargement of the soft septic spleen; and (3) the absence of a noteworthy diminution of the weight of the animal, at least during the first feverish attacks. Concerning the first-named condition, it is true that, in the graver forms of malarial infection in men, the fever often enough takes on a continued character, or has it from the beginning (*perniciosa*); so that in the animal also a continued fever would not perfectly exclude the existence of malarial infection. Another reason of some moment, tending to show that domestic animals generally are unfitted for experiments on malaria, seems to be their oft-observed immunity from intermittent fevers in malarial districts. Still, this is not absolutely proved; and the authors assume, on somewhat hypothetical grounds, that there might exist in the animals special protecting conditions against the infection with germs. But we shall soon see that the scruples in that direction have been unfounded. Lastly, we have to note another important kind of evidence for the malarial infection of animals; that is, the existence of the characteristic black pigment in the spleen, the liver, the marrow of the bones, and the blood—a pigment which contains iron in inorganic composition (recognizable by the blue colour after treatment with ferro-cyanide of potassium and hydrochloric acid—a reaction which is not to be obtained with hæmoglobin). From the existence of iron in that form it follows that during life, and in the interior of the otherwise unaltered blood-corpuscles, the hæmoglobin becomes decomposed and its iron set free by the influence of the malarial poison.

When the authors had been so far successful as to produce by injections of watery extracts from the marshy soil functional disorders and anatomical lesions in animals, which were completely in accordance with those following the natural infection with malaria, the task next to be undertaken was the isolation from the crude material of the active substance—that is, the true generator of the disease. If that germ belonged to a plant, it would be possible to isolate it from the accompanying organisms by cultivating it from the crude material in a medium suitable for its further development; for then the other organisms will gradually disappear. This method of fractional culture (*fractionirte Cultur*), applied successfully by Professor Klebs in other instances, gave the desired result in this case also. Lastly, it would be necessary to apply filtration for isolating the

organisms absolutely. By injecting the liquid containing the organisms, and by comparing the results afforded by that injection with those following injection with the filtered liquid, it can be proved that the special organism alone is efficacious, whilst the second injection has no effect.

Besides those organisms which were taken from the marshy soil, the authors also cultivated germs in a special way, which they had collected from the air above the soil. At some places specially exposed to the malaria a large quantity of air taken from a very small distance above the ground was sucked through a ventilator of special construction, which permitted the collection of all corpuscular elements contained in the air on a comparatively small space—that is, on a small glass plate covered with a mixture of glycerine and isinglass. Examination with the microscope showed, amongst those particles of dust, rods in very small number and filiform elements of some length. When those elements were transmitted into fresh urine, free from germs, an abundant development of bacilli was shortly to be seen, which showed the same characters as those cultivated from the soil. It was possible, after repeating the breeding process several times in different mediums at a definite temperature, to obtain a development of schizomycetæ which showed constantly the same characteristics, whether the germs had been taken from the soil, the marshy water, or the air. These fungi appeared as small rods of 0.002 to 0.007 millimetres in length, growing into long twisted threads, which after some time became furnished with joints at the surface of the liquid exposed to the air, or formed the so-called enduring germs (*Dauersporen*) in their interior. Besides the characteristic form of the fungus, it is distinguished from other bacteria by its relations to the air. It is markedly aërobiotic. If air has no free access, or is excluded, it will not grow at all, or dies out. Inasmuch as the development of the fungi was so constant, it was probable that this organism was the true *bacilus malariæ*. The next problem that arose was to test its nature by injecting it into the living animal.

Before the experiments on the animals could be begun, it was necessary to determine exactly the dimensions of the spleen as well as the average normal temperature. This done, the experimenters were able to define more exactly the pathological conditions of the injected animals.

In the first group of experiments two rabbits were injected with organisms collected from the slime and the air of the Pontine Marshes. Every one of them showed intermittent fever after one injection. When they were killed they presented very marked enlargement of the spleen, which contained large masses of dark brown pigment. The spleen and the lymphatic glands contained very small, bright corpuscles, which developed, after twenty-four hours in a suitable medium, into threads filled with spores. Formation of pus or any other changes due to inflammatory or septic processes of the different organs were entirely absent.

In further experiments, cultivated bacilli taken from urine, or the isinglass mixture, furnished the material for the injections, and always with the same positive results. The soil, taken from different places in Rome itself and the Agro Romano, proved, in nearly every instance, efficacious also. But the injection with water standing over the marshy ground remained without effect.

Of course it was necessary also to try soil free from malaria. Professor Klebs took infusions of the soil of the garden in the pathological laboratory at Prague. The results remained in this case somewhat doubtful, because the animals became ill, but not in the same way as before, so that it seemed that a mere septic injection had taken place. Two rabbits became ill and died with symptoms of septic poisoning spontaneously at Rome, so that it was possible to compare the two processes directly; and the pathological conditions found in the dead bodies

proved different in these latter cases, because pus was found, pigment was totally absent, and the spleen, although enlarged, had lost its firmness.

Afterwards Dr. Marchiafava at Rome was able to demonstrate spores and bacilli in the spleen, the marrow, and the blood of three persons who had died of pernicious fever, showing the same characters as those observed by Klebs and Tommasi-Crudeli.

In summarizing the results of their investigations, the authors consider the following facts as proved: (1) That it is possible to reproduce malarial infection in every form in rabbits in which it is known in men; (2) that the malaria produced artificially in animals is generated by organisms existing in the malarial soil at the time when the outbreak of the fever has not yet taken place. We see, therefore, that those experiments, although small in number, have led to very important results. It is to be hoped that they may become the starting-point for further investigations in the same direction. As the authors themselves allow, there are still many points which require further elucidation. But it is encouraging to see how, by a comparatively small number of careful experiments, good results can be arrived at, when a strictly logical and methodical course of investigation has been pursued. — *Med. Times and Gazette*, Jan. 17, 1880, from *Archiv für Experimentelle Pathologie und Pharm.*, Bd. xi.

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*On the Pathology of the Central Nervous System.*

O. Kahler and A. Pick publish a series of contributions to the pathology and pathological anatomy of the central nervous system in the *Prager Vierteljahrschrift*, new series, 1879, pp. 1 and 2. The first is a contribution to the localization of functions in the brain. According to Wernicke, the central end of the auditory nerve and, with special reference to aphasia, the centre of formation of sounds, are to be placed in the first temporal convolution; and lesion of the same must be considered the fundamental cause of deafness for words (Kussmaul). The authors communicate the following case in evidence. A woman, 42 years of age, suffering from aphasia, who could utter but very few intelligible sounds, but who presented no signs of paralysis in the extremities, could hear, without understanding what was said to her. She had to be guided for the simplest offices by the nurse, as oral directions were unintelligible to her. On making a *post-mortem* examination, the convolution of the left temporal lobe appeared soft and gelatinous, as did the posterior half of the third frontal convolution. The right temporal lobe was also softened to a smaller extent. A second case was that of a drunkard, aged 37, who was paralyzed on the right side, aphasic, and deaf to words, though he heard plainly. A hemorrhagic pachymeningitis, chiefly on the left side, had compressed the whole brain, especially the middle and third frontal convolution, almost the whole temporal lobe, and the lateral portions of the parietal and occipital lobes. As to those cases recorded in which, in spite of affection of the temporal lobes being shown afterwards, no trace of word-deafness was observed, there was (according to the authors) in every case a sufficient lapse of time for the recovery of the possibly lost power of understanding words. In connection with this, the authors mention, with regard to the first case (in which during life no lesion of the nutrition or of the motor power of the extremities was shown), the actual presence of a myelitic focus in the spinal cord in the left anterior cornu of the lumbar region, by which, besides the local changes, asymmetry of the whole cord was produced. This shows the necessity of care in investigating such cases of asymmetry, and of not omitting to make a thorough search for the presence of some myelitic focus, however small, in the spinal cord.

In the second section, an interesting case of word-deafness (*Wortaubheit*) is communicated; that of a man, aged 55, who was suffering from atactic aphasia, word-deafness, agraphia, partial ataxy, slight loss of power in the joints, and loss of musical memory.

The third section treats of the localization of motor-phenomena after hemiplegia. The authors observed in a man, aged 60, about six months after an apoplectic attack, which caused paralysis and complete anæsthesia of the right half of the body (including the sense-organ), purposeless movements of the finger and hand on the paralyzed right side. Later on, twitchings and violent swinging of the right arm and uncontrolled movements of the toes set in. These movements ceased during sleep, but the hand always assumed a peculiar position (super-extension and abduction of single fingers). At the posterior end of the left corpus striatum, an opening led into an apoplectic cyst as large as a farthing. The same was found in the external part of the optic thalamus, and it sent a prolongation outwards through the inner capsule. On vertical section through the middle of the thalamus, the apoplectic cicatrix was seen in the outer half of the thalamus, and extended as a reddish-brown colouration through the whole breadth of the inner capsule to the nucleus lenticularis. This place lay immediately behind and bounding that which, according to Flechsig, contains the compact bundle of the fibres of the pyramids. A partial secondary degeneration was found in the right lateral column of the cord. The authors consider the lesion of the compact bundle of the fibres of the pyramids, which ascends between the optic thalamus and the posterior part of the nucleus lenticularis, to have been the cause of the pathological appearances; but that it is also possible that lesions of the fibres of the pyramids in other places (as the inner part of the capsule) may be followed by the same, or similar results.

The fourth section is on ataxy, and ataxy after acute diseases. A case of cerebral ataxy is interesting, showing that circumscribed disease of the motor tracts of the cortical substance of the brain may cause crossed ataxy. It was that of a woman suffering from tuberculosis, who first experienced a feeling in the right hand and arm as if ants were running over it; and soon after this a difficulty in using the limb. Though the sensibility of the skin was unimpaired, all sense of the position of the limb in space was lost. Later on, headache, aphasia, and paralysis of the right side set in. After death, along with tubercular meningitis of the base, a flat cheesy tumour was found in the posterior part of the left frontal lobe, and a second smaller one on the parietal lobe. Similar results have been described by Bernhardt, Gelpke, Fritsch, Hitzig, and Nothnagel, in their experiments upon excision of the cortical substance of the brain; and recently by Munk. As the authors (like Friedrich) believe that the destruction of co-ordination of movement is to be referred to lesions of the cortical substance of the cerebrum, cerebellum, medulla oblongata, or spinal cord, there remains a series of cases of ataxy, which until now have wanted any distinct anatomical proof of the place of the lesion in the cerebro-spinal system. These are the cases of acute or functional ataxy after acute illness. The authors group the recorded cases and some new ones under three heads: 1. Cases with more or less rapid recovery; 2. Cases ending in chronic incurable disease; 3. Cases rapidly leading to death. To the first group belong cases of acute ataxy after intermittent fever (communicated by the authors), pneumonia, erysipelas of the face, scarlatina, measles, typhus, and diphtheria. Most probably, in all these cases, there is a wide-spread pathological process independent of the nerves, which can only, in a few cases, be more accurately localized, as by absence of the patellar tendon-reflex in the lumbar cord, by aphasic conditions in certain regions of the cerebrum, etc. To the second group belong especially the cases following varioloid and

typhus, described by Westphal and Otto (*Centralblatt für die Med. Wiss.*), and a case likewise after typhus, observed by Kahler, and the important case described by Ebstein (*ibid.*, 1872, p. 367). There proof was given that, after abdominal typhus, an illness may arise, causing after several years small sclerotic foci in the medulla oblongata and medulla spinalis. In the third group, that of cases with fatal results (Leyden, Jolly, Oertel), the condition was that of extensive acute myelitis, from which, as Leyden showed, sclerosis may arise (*Centralblatt für die Med. Wiss.*, 1878, p. 676). In cases of the first group, with quick recovery, the authors assume that there has been an acute illness caused by the migration of fungi, which, in consequence of the diminished resisting power of the organ attacked, that is to say, in conditions favourable to the development of the fungi, may lead to myelitis and sclerosis.—*London Med. Record*, Feb. 15, 1880.

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*Cases Illustrating the Connection between Phthisis and Brain Disease.*

Two cases of lung disease, with secondary brain complication and paralysis, published early this year by Professor SENATOR, of Berlin ("Zur Diagnostik der Hirnerkrankungen"—*Berliner Klin. Wochenschrift*, Nos. 4, 5, 6, 1879), and to which our attention has recently been directed, are of so interesting and unusual a character that an abstract of them cannot but be acceptable to our readers even now. Later on in this article we shall bring forward some other cases belonging to the same category, but more interesting from the light they throw on cerebral localization than from their etiology.

The first case which Professor Senator describes is that of a man, aged thirty, admitted under his care on July 31, 1878, with dulness of the upper lobe of the right lung, bronchial breathing, not very abundant bubbling râles of medium size, cough, and rather scanty expectoration of a somewhat putrid character. With the exception of some fever in the afternoon, and occasional diarrhœa, his general health was as yet but little impaired, and he did not at any rate require to keep his bed. The aspect of affairs soon altered. On the 4th of August he first noticed that his right hand was weak, and that he dropped his spoon at meals. On August 5 it was found that all the fingers of this hand were partially paralyzed, and could be neither flexed nor extended properly. There was no alteration of the sensibility of the skin, but the whole right hand and forearm were warmer than the left, and perspired freely. The next day voluntary movements of the hand were quite abolished, and the forearm seemed somewhat œdematous in comparison with the left.

The forearm muscles proper became paralyzed on the 7th of August; on the 8th the patient had an epileptiform fit with loss of consciousness, and beginning with twitchings in the right arm. These fits recurred on the two following days with different degrees of intensity, the right side of the body being alone convulsed, with scarcely impaired consciousness in one of them. On August 11 the muscles supplied by the lower branches of the facial nerve were paralyzed; in the next two days the power of voluntary movement of the right leg was lost. Sensation everywhere remained intact, but the patient gave an inaccurate account of the position of his limbs. On August 14 symptoms of aphasia appeared, and on August 16 he only answered "yes" or "no" to questions, and that not always coherently. From this time he gradually became apathetic, and then comatose, and died on August 19. The electrical irritability of the arms was found the day before death to be the same on the two sides, but the circumference of the right arm on measurement was about half a centimetre less than that of the left; whereas on August 7 the latter was a centimetre the smaller. The ophthalmoscope gave no clue to the nature of the brain disease. The pulse was not re-

tarded and ranged between 92 and 112. The highest evening temperature was 39.4° C. on August 13; the highest morning 39.3° on August 14.

A cavity was detected at the right apex some days before the patient's death. We pass over the details of the post-mortem examination of the phthisical lungs, to give a brief note of the state of the brain. In the left hemisphere an abscess was found, containing about fifty grammes of foul pus, and extending from close under the pia mater to about half a centimetre from the roof of the lateral ventricle. The corpus striatum and optic thalamus were intact. The exact position occupied by the abscess, as far as could be made out, was the region of the ascending frontal convolution,[1] and especially where it borders the fissure of Rolando, and a small part of the neighbouring first and third frontal convolutions. The ascending parietal convolution was slightly discoloured and flattened on the side towards the fissure of Rolando, but otherwise scarcely affected by the abscess.

There was no sign of any old or recent injury to the skull. Except a couple of spoonfuls of serum in the ventricles, the rest of the brain—pons, crura cerebri, and medulla oblongata—were healthy, as well as the bloodvessels at the base.

The diagnosis of the disease was doubtful during life. It lay between cysticercus (the patient having once suffered from tapeworm), tubercle, and abscess. The post-mortem showed that the first two contingencies were inadmissible. The absence of a rigor, Senator specially observes, must not be taken as negativing the existence of abscess.

The great interest of this case arises, first, from the connection between the abscess and the ulceration of the lung; and secondly, from the nervous symptoms produced by the peculiar situation of the abscess. The possible dependence of cerebral abscess on lung disease, first pointed out in 1858 by Sir W. Gull, has since been insisted on by Biermer, R. Meyer, and Huguenin; but from 1867, when Meyer collected and published eighty-nine cases of all forms of this affection, eleven of which were complicated with suppuration in the lungs, until the end of 1878, only ten cases had appeared in the English and foreign medical journals in which the reporters had mentioned the existence of pulmonary disease, and in several of these they only spoke of it casually, as if it could have no possible bearing on the production of the abscesses. Senator gives references to twenty-six cases of abscess of the brain, published between 1867 and 1868, in which are included the ten above referred to, the remaining sixteen being described by the writers as "idiopathic." This fact alone shows how important it is that the attention of the profession should be directed to the relation of pulmonary disease to cerebral abscess. The physiological researches of Hitzig, Ferrier, and others, have left little doubt that part of the cortex of the brain has motor functions, and the results of pathological observation every day add confirmation to this view. Having recently spoken of this cortical centre in reviewing Dr. Decaisne's work "Des Paralyses Corticales du Membre Supérieur" (*Medical Times and Gazette*, November 1, 1879, page 511), we need say little further with regard to cerebral localization in Dr. Senator's case than that the absence of lesion of the left ascending parietal convolution, with complete

[1] Professor Senator speaks, in first describing the relations of the abscess, of the "second frontal convolution" (*zweite Stirwindung*) as the most involved (*loc. cit.*, page 43), but it is clear, not only from what we know of the "motor centre" of the cortex, and also from his own after-statement (*loc. cit.*, page 61), that "the motor disturbances must be explained by the disease of the ascending frontal convolution (*vorderen Centralwindung*)," that he was in error in speaking of the second frontal convolution.

paralysis of the right upper and lower extremities, is noteworthy. Certain peculiarities in the symptoms observed in this case deserve careful attention—(1) The patient had disturbance of the muscular sense, so that he was unconscious of passive movements or changes of position in his right arm and leg. The same thing has been observed by Hitzig, Nothnägel, and Munk, after extirpation of the corresponding region of the brain in animals. The only other published case where this symptom was observed, and which was verified by an autopsy, is, as far as Senator knows, that of Gelpke (*Archiv der Heilkunde*, 1876, S. 418). (2) His right arm was warmer than the left; it perspired more vigorously, and for one or two days was slightly œdematous, while its circumference diminished to a measurable degree during the short time the patient was in hospital. Senator believes that his is the only known case in which *cortical* disease of the brain and sweating on the paralyzed side have been noted together. In disease of the large ganglia, from hemorrhage, etc., sweating is, of course, by no means so very uncommon. The experimental results of Bochefontaine, Hitzig, and others, who found that extirpation or irritation of certain parts of the motor region of the cortex were followed by a rise of temperature in the opposite limb, are thus strengthened, if not confirmed, as are also those of Luchsinger, Nawrocki, and Adamkiewicz (*Medical Times and Gazette*, October 25, 1879, page 486), with reference to a cerebral centre of the sweat function. The apparent rapid atrophy of the muscles of the right arm and forearm in the last ten days of the patient's life, Professor Senator regards as due to a possible injury of the "giant" nerve cells discovered by Betz in the motor region of the brain (*Medical Times and Gazette*, vol. ii., 1874, page 498), and which may perhaps have trophic functions analogous to those of the large cells in the anterior cornua of the spinal cord; but this is a question which cannot be settled without much further investigation. It is nearly certain, however, that the atrophy cannot be accounted for in this case by disease of the particular muscles, for, though paralyzed to the will, they were repeatedly violently contracted during the epileptiform seizures. With reference to these seizures, the fact that at first they could be abated by drinking salt and water, or by tying a band round the arm in which the twitchings began, is interesting, though of course frequently observed in true epilepsy, and of late in cortical disease (Charcot and Pitres, and others).

Senator's second case may be dismissed much more briefly than the first. Again, we have paralysis beginning in the right hand, extending to the lower branches of the facial nerve, then to the whole arm, and lastly to the right leg, in a man of twenty-nine, with phthisis and cavities in the left lung, the patient dying within eight days of the first onset of nervous symptoms. As in the first case, general sensibility was unaffected, but no convulsions occurred, and there was no aphasia. The bladder and rectum were not paralyzed in either case. In the second, consciousness was preserved to the last. The post-mortem examination differed very materially, however, in the two cases, for in the second *no localized disease could be detected anywhere in the brain*. The most careful search revealed nothing but considerable dilatation of the bloodvessels of the substance of both hemispheres—if anything, more left than right,—general injection of the pia mater, and fulness of the large veins. Professor Senator calls attention to the onset of lobar caseous pneumonia with marked cyanosis in the last fortnight of his patient's life as possibly explaining the cerebral symptoms. Assuming coincident, but unevenly distributed, cerebral hyperæmia, it is not unlikely "that the motor centres, or tracts in the left hemisphere, were specially compressed, although no anatomical proof of this could be discovered." Cases like the above are, as Senator observes, very scantily represented in medical literature. Graves and Andral have referred to one or two of doubtful resem-

blance to it, but the nearest approach to it is one by Romberg and Henoch ("Clinische Beobachtungen;" Berlin, 1851, S. 45). Since Professor Senator published his paper, three other German physicians have recorded cases of phthisis with brain symptoms and paralysis,—Dr. Goltdammer ("Kasinstische Mitheilungen zur Pathologie der Grosshirnrinde"—*Berl. Klin. Wochenschrift*, Nos. 24, 25, 1879), Professor Drasche, of Vienna ("Ein Fall corticaler Epilepsie"—*Wiener Med. Wochenschrift*, No. 39, 1879), and Dr. Bernhardt, of Berlin ("Klinische Beiträge," etc.—*Archiv für Psychiatrie*, Bd. iv., S. 698), the reference to whose paper we owe to Professor Drasche. Their cases, however, have no bearing on the connection of phthisis and abscess of the brain, for the lesions met with in the brain were tubercular. They interest us mainly because of their bearing on the subject of cerebral localization and the motor centre. Dr. Goltdammer's patient, a man of forty-two, with advanced phthisis, was seized ten days before death with headache, followed by epileptiform convulsions involving the left arm and left facial muscles, with loss of consciousness. Soon afterwards the left arm and the parts supplied by the buccal and ocular branches of the facial nerve became paralyzed. The paralysis here never extended to the left leg, and the patient quickly died with high fever and coma. The right ascending frontal and parietal convolutions were found flattened, pale, and broader than normal, with the pia mater over them thickened and the seat of miliary tubercles. The gray substance of the median portion of these convolutions was softened at three spots, each about the size of a pea, and a deep section revealed a nodule of hemorrhagic softening as large as a walnut near the middle of the fissure of Rolando. The softening was probably the result of obliteration of vessels, partly by tubercles in their adventitia, and partly by the compression of the thickened pia mater. The rest of the brain was healthy. Professor Drasche's case only differs from the last in the leg as well as the arm and face becoming paralyzed. Here also there was loss of consciousness. The patient was again a male, aged twenty-four. The pia mater covering the ascending convolutions was found studded with caseous tubercular nodules, and there were gray miliary tubercles in the Sylvian fissure. The gray substance of the particular convolutions was softened. We have not been able as yet to read the details of Dr. Bernhardt's case, but it is nearly identical, according to Professor Drasche, with his and Dr. Goltdammer's. It will be clear, even from the meagre description we have given of these latter, that they by no means rival Senator's two cases in physiological or pathological interest. The termination of phthisis in tubercular meningitis is no rarity; it is only the special localization of the tuberculosis and its products which arrests our attention here. Senator's cases stand out from the rest, especially his first, by the association of a rise of temperature with sweating in the paralyzed arm, and by the curious affection of the muscular sense and possibly of the trophic nerves of the limb. It is these little differences between the symptoms which mark a series of roughly similar cases that should make us pause and think whether the cortex of the brain is not even more complicated than many persons at present imagine. It seems not unlikely that in one and the same convolution there may be a series of ganglionic centres, each controlling a separate function, just as we have long known of the existence of such ganglia or "nuclei" in the floor of the fourth ventricle. It is better, however, to accumulate facts than to speculate prematurely.—*Med. Times and Gazette*, Jan. 17, 1880.

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*Inhalation of Benzoate of Soda in the Treatment of Phthisis.*

Great interest has been excited in Germany by alleged cures of phthisis by the inhalation of pulverized solutions of benzoate of soda. So great, indeed, was the

excitement produced by the first published account of the cures effected in quite desperate cases, by Professor Rokitansky of Innsbruck and his assistant, that the political journals rang with the reports of this marvellous discovery, society was greatly stirred by it, and the echoes of it have been noticed in paragraphs current in our newspapers; various queries have also been addressed to us on the subject. We have thought it well to wait until the test of further clinical experience has been applied. This has now been done, with, unfortunately, decisive negative results, especially by Dr. Guttmann of Berlin.

The history of the matter begins with a communication, made by Professor Klebs, at a scientific meeting in Munich, in June, 1877, when he announced the opinion, founded on experimental and microscopical observations, that tuberculosis was an infectious disease of parasitic nature; that it was induced by certain micro-organisms which invaded the body and multiplied in it; and that the expectation might be entertained of curing it by the employment of means which are calculated to annihilate these organisms. The experimental proof of the parasitic character of tuberculosis was conducted by Klebs in this wise. If the smallest piece of tubercular matter—for example, a fragment so minute that it can hardly be seen by the naked eye—be put into a vessel with a suitable nutritive fluid, such as a solution of white of egg, the solution, after a short time, becomes turbid, from the development in the fluid of a great number of micro-organisms, which show lively movements. In a control experiment, in which the same solution of white of egg is put into a similar vessel, but without the addition of tuberculous substance, the solution remains quite clear. Cultivation of successive solutions of white of egg, to which only a drop of the first tested solution is added, leads to the infection of each in turn, if a drop of the first be added to a second, a drop of the second to a third, and so on. Further interest attaches to this theory on the origin of tubercle, in consequence of the recently published researches of Dr. Schüller of Griefswald. These researches show that animals, into which tuberculous masses have been inoculated by tracheotomy, or who have been injected with micro-organisms obtained by fractional culture, according to the method of Klebs, died without exception with very marked phenomena of emaciation, loss of heat, and, after a little time, general miliary tuberculosis. When, however, the same animals, after infection with an equal quantity of tuberculous substances, and the bacteria obtained from them, and after the appearance of the characteristic phenomena of disease, as in the previously mentioned animals, were subjected to the inhalation of a pulverized solution of benzoate of soda for some weeks, they all survived. Their body-weight rapidly recovered itself; growth of hair became stronger; and, after a short time, the animals showed again their normal conditions. In passing, it may be mentioned also that Dr. Graham Brown had found, in the course of researches in the laboratory of Klebs, that when the fungi of diphtheritic membranes were treated with a solution of benzoate of soda, or when animals were injected with these fungi which had been saturated with a solution of benzoate of soda to the extent of a thousandth part of their body-weight, the diphtheritic fungi did not grow, and became inactive.

On the basis of these results of research, a five per cent. watery solution of benzoate of soda was employed, in the hospital at Innsbruck, by pulverization, as an inhalation in cases of phthisis. In a communication of Dr. Kroczak, assistant to Dr. Rokitansky, to the *Wiener Medizinische Presse*, September 14, 1879, favourable results were announced which had been obtained in fifteen patients so treated, "the results exceeding all expectations." The three most severe of these cases were described in detail. Two showed "generally diffused local phthisis, with the formation of caverns." The third, also a case of exten-

sive phthisical disease, was complicated with well-marked pleural exudations. These three were only put forward as examples of cases treated, and were, as was stated in so many words, "persons, so to say, moribund upon entrance to the hospital, and who were sent out cured." All these patients were already free from fever at the end of a week; the third after ten days; and the weight of the body also very quickly increased. It is not stated how long the treatment by inhalation was carried on; but it could scarcely have been longer than three weeks, according to the dates given at which the patients entered the hospital and left it cured.

Dr. Guttmann, in a communication which he made to the Berlin Medical Society in November, and which was recently published in the *Berliner Klinische Wochenschrift*, in recalling these preliminary facts, observes also that Professor Schnitzler of Vienna had arrived lately, from a series of experiments (*Wiener Medizinische Presse*, 1879, No. 42), at the conclusion that no medicament enters the bronchi by the inhalation of pulverized medicated solutions. Guttmann, however, points out that these conclusions are a little too absolute, inasmuch as well-known pathological observations show that, after continuous exposure to an atmosphere loaded with particles of coal or iron, or other coloured substances, the lungs are found after death to be impregnated with such materials. While it seems clear that the greater part of the substances pulverized in medicated solutions are deposited on the tongue, pharynx, soft palate, and epiglottis, it seems nevertheless, beyond doubt, that small particles pass the rima of the glottis, and to a small extent enter the trachea, and possibly pass into the bronchi. Of course, such results are obtained much more completely when the patient can be retained permanently in the inhalation-chamber. Dr. Guttmann's treatment extended over thirty-one phthisical patients, of whom twenty-four were men and seven were women, from seventeen to fifty-six years of age. The phthisis was in the majority of the cases very extensive, often having proceeded to the formation of considerable cavities; in a smaller number of cases, the disease was relatively lighter—cases which were at times either altogether free from fever, or which were running their course with only slight fever. Those patients were purposely chosen for this observation whose temperature showed a well-marked hectic type, that is to say, low morning temperatures, high evening temperatures, and moreover who, during the latest part of their stay in the hospital, had shown a certain steadiness in their temperature-curves. Any influence of the treatment by inhalation could, therefore, be very easily observed in the temperature-curves of such patients. The temperature was taken three times a day at the same periods—morning, midday, and evening. The inhalations took place twice daily—morning and evening. The solution of benzoate of soda was a five per cent. solution in water. At first, Dr. Guttmann prescribed only five *grammes* of benzoate of soda daily, that is to say, the inhalation of one hundred *grammes* of solution; later, he rose, in the case of five patients, to ten *grammes* of the substance, that is to say, two hundred *grammes* of the inhaled solution a day. When Rokitansky's communication appeared, in which he intimated that his patients had daily inhaled the one-thousandth part of their body-weight of benzoate of soda in substance, that is to say, fifty *grammes* of benzoate of soda in substance per fifty *kilogrammes* of body-weight, and therefore a thousand *grammes* of the solution, then Dr. Guttmann also raised the prescriptions of the patients to these large quantities. Of the thirty-one patients, fifteen inhaled during three weeks; one for two days (the latter had during this time consumed seven hundred and ten *grammes* of benzoate of soda in substance); six patients inhaled during from fourteen to nineteen days. Of these, one took six hundred and fifty *grammes* of benzoate of soda in substance. The remaining nine patients

inhaled during from three to twelve days. In four of these who did not tolerate inhalation, the benzoate of soda was given internally, in the quantity of from twenty *grammes* of benzoate of soda to two hundred of water (one tablespoonful every two hours). Of these thirty-one patients, nine died—seven men and two women; two were allowed at their own wish to leave the hospital; and the rest were still there. The result obtained was as follows: In not one case—not even, therefore, in the patients who inhaled daily a thousand *grammes* of the solution—was the temperature in any observable manner lowered by the benzoate of soda, or was any influence exercised on the temperature-curves. Dr. Guttmann adds that the internal use of the benzoate of soda, during a considerable time in which a daily quantity of about eight *grammes* in substance was taken, was equally without influence on the febrile temperature. On the second notable factor in the progress of phthisical patients, improvement or falling off in weight, the treatment with benzoate of soda was equally without influence. In most cases, the weight of the body fell off progressively, in proportion to the amount of pyrexia and exactly in the same manner as though the patients had not been undergoing treatment during this time. Only in one or two patients, in whom the fever was throughout and from the first slight, did the body-weight remain stationary or slightly increased. Neither did the local symptoms of the phthisis undergo any change during the course of the treatment, in the sense of improvement. Dr. Guttmann observed, indeed, that the catarrh and bleeding diminished shortly after the inhalations; that is to say, that the patients coughed and expectorated less for about an hour after the inhalations were over. This, however, he attributes to the fact that the inhalation, and the continuous deep inspiration required while it was going on, excited during the time a much more considerable amount of coughing by irritation; and thus, during the period of inhalation, a much more considerable amount of expectoration was induced—thus clearing the cavities and tubes, and giving rise to less expectoration for a short time after the inhalations had ceased. There was no permanent improvement either of cough or of expectoration after the inhalations, nor were the nocturnal sweats, when these were present, at all lessened by the benzoate of soda. While in not one case was any symptom of phthisis observed to be improved by inhalation; on the other hand, an occasionally unpleasant incidental action of the benzoate of soda was observed. A good many patients complained of nausea and some of vomiting; symptoms which, partly at least, were excited by the continuous stretching forward of the tongue during the inhalation, on which Rokitansky lays considerable stress, in order that the inhaled substance might reach the air-passages. It appeared to be partly due to the fact that the benzoate of soda acts as an excitant on the mucous membrane of the stomach, in so far as it reaches it by the inhalations through the œsophagus, and so onwards. Guttmann has observed in one case, on *post-mortem* examination of a phthisical patient who had inhaled benzoate of soda quite up to the end of his life, a recent and general capillary congestion of the mucous membrane of the stomach. In the *post-mortem* examination of nine phthisical patients, who had inhaled benzoate of soda, nothing was found in the pathological condition of the lungs which in any way indicated the commencement of a healing process. There was generally an extensive phthisical change, with cavities filled with fluid pus. In one case of left-sided pneumothorax, there was a perforated cavern. Two phthisical patients, who had for several years shown no symptoms of hæmoptysis, were attacked freely with hæmoptysis after inhalations. These researches, therefore, carried on during the same period, in the same manner as those which were published from Rokitansky's clinique, are in absolute and complete opposition to them.

Dr. Wenzel reports also ten cases of phthisis, treated at the Charité Wards at

Berlin, under Professor Waldenburg, with benzoate of soda inhalations, under similar conditions to those of Dr. Guttmann. He also reports, as the result of treatment, that neither in the more severe nor in the lighter cases was any direct effect produced on the phthisical process. There was not once even any symptomatic or palliative improvement. Four of them ended fatally during the three or four weeks in which the treatment was being carried on. There was no effect upon the febrile process, nor was the appetite improved. The inhalations for the most part were well tolerated, except that in two cases they produced slight nausea and vomiting; and many of the patients complained of the inconveniently long inhalations. On the whole, the results of the experiments in Dr. Waldenburg's wards were not at all more favourable than those of the other cases.

In the last number of the *Wiener Medizinische Wochenschrift*, Professor Drasche reports, from the General Hospital at Vienna, the results of his employment of benzoate of soda in twenty cases, and these results are equally disappointing. The effect of the benzoated inhalations appears to have been quite without palpable influence upon the well-being of the patients. Six out of the twenty died; three of them were on the point of death; six were neither better nor worse; and, altogether, the results were entirely hostile to those announced by Rokitansky and Kroczak. Moreover, Drasche observed one very significant case, in which, at the beginning of the benzoate treatment, the larynx was quite intact, but, under the continuous inhalations, reddening and swelling of the vocal cords commenced under his eyes, went on to hoarseness, infiltration, and, finally, ulceration on both sides, notwithstanding the continuous moistening of the larynx by this pulverized solution of the benzoate of soda. This fact is, he observes, a very significant comment on the theory, that physical ulcerations are due to the excessive development of the infective bacteria, which benzoate of soda is supposed to destroy. The whole question of the application of disinfectant inhalations to the lung is treated, in its historical relation, in Waldenburg's book, *Die locale Behandlung der Krankheiten der Athmungsorgane;* and it is, of course, one of the oldest and most obvious methods of treating lung disease. As to the physiological action of benzoate of soda, apart from its antimycotic power, for which it was recommended by Klebs and employed by Schüller and Rokitansky, in this instance it may be observed that Salkowski, who, in 1875, first called attention to the antiseptic action of benzoate of soda, points out now, in Virchow's *Archiv* (vol. 78, part 3), that the elimination of nitrogen appears to be considerably increased by it, and that an increase of the destruction of the albuminoids of the body is caused. The increased excretion of nitrogen extended to five *grammes* daily; and, in the course of a week, by a continuous use of high doses of benzoate of soda, a loss of one *kilogramme* of muscle would be caused. These exact researches put Rokitansky's observations of an increase of body-weight, in certain of his phthisical patients, in a special light; since it is clear that, if such increase occur, it must be rather as the consequence of the improved nutrition of the patients after being taken into hospital from poor homes, than of any arrest of the tubercular disease, which does not seem to be affected by the benzoate of soda, or of any direct action of benzoate on the body, for such action would evidently be in an opposite direction.—*British Med. Journal*, Jan. 3, 1880.

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### *Cardiac Lesions in Chronic Interstitial Nephritis.*

In a note presented to the Société Anatomique, and published in the current number of *Le Progrès Médical*, Drs. DEBOVE and LETULLE direct attention to the constant presence of fibroid overgrowth in the papillary muscles and wall of the hypertrophied left ventricle in chronic Bright's disease. They assert that

such a condition has been hitherto overlooked by writers; but it is fair to state that it has not escaped observation, although the significance they give to it has, perhaps, not hitherto been dwelt on. Indeed, it may safely be said, as a matter of common post-mortem experience, that considerable muscular hypertrophy of the ventricle is associated with patches of limited fibroid change, and this wholly apart from any chronic valvular lesion. The present writers show, however, that the sclerosis takes place at the expense of the muscular elements, which appear shrivelled and wasted in the vicinity of the change, and they go so far as to attribute the final symptoms of chronic Bright's disease to this cardiac condition. Thus they say that the first period of interstitial nephritis—the renal period—is characterized by slight albuminuria, polyuria, and slight and transitory œdema. But in the second, or "cardiac stage," palpitation and dyspnœa supervene, with oliguria, considerable albuminuria, and increasing anasarca, with dropsical effusions. Whatever truth may exist in these suggestions, they are certainly open to the criticism that no notice is taken of the increased impairment of the renal function, with the altogether strange omission of any reference to uræmic poisoning as an element in the production of some of the symptoms. The note is too brief to admit of extended or minute criticism, and we are glad to see that the authors promise to continue their investigations, and to enter more fully into the subject in a memoir shortly to be published.—*Lancet*, Jan. 3, 1880.

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*Passage of a large Gall-stone per Anum.*

At a late meeting of the Pathological Society of London, Dr. Ord showed a large gall-stone which had been passed per anum by a lady under the care of Dr. Carr Roberts. She had been recently confined of a living child, and the only symptoms at all referable to the calculus were excessive pain in the back, and constant diarrhœa of a pale-yellow colour; there was no jaundice. The stone, which was removed by the finger, measured 1⅝ inch in its long diameter, and 1¼ inch across. It was composed of cholesterine and bile pigments, and was therefore clearly not an enterolith, which consists of hair, concreted indigestible matter, and triple phosphates, lime salts, eleagic or litho-fellinic acid, but never of cholesterine. Referring to the structure and composition of gall-stones, Dr. Ord pointed out that they consist chiefly of a mixture of cholesterine with biliary pigment, mucus, lime salts of biliary acids and carbonate of lime being occasionally present. On section, they are seen to consist of four parts: a dark central portion, consisting mainly of pigment, with a small quantity of cholesterine in rhomboidal forms; around this a lighter portion with a distinctly radiated structure, composed of columnar masses of colourless cholesterine, with biliary pigment packed between; surrounding this is a zone, with the laminæ arranged concentrically, and composed of undulating crystalline fibres of cholesterine stained with pigment, more deeply stained towards the surface; the outer surface of the calculus, which is also the most recently formed, is deeply coloured, without any definite structure, and is composed of an intimate mixture of cholesterine in shapeless masses, spheres, or fibres, with biliary pigment. These facts point to a gradual metamorphosis of the calculus, and Dr. Ord suggested that it was the presence of the colloidal pigment which prevented the freshly deposited cholesterine from assuming at once its proper crystalline form; but that, as the pigment tends to become itself crystalline, the two bodies gradually separate, and the cholesterine crystallizes. In support of this hypothesis, it was alleged that in the most deeply pigmented calculi the outer layer is abundant, while in the paler ones the crystalline centre predominates. Dr. Ord related some experiments in which he found that cholesterine deposited in albumen or gelatine is at

first spherical, and gradually assumes a quasi-crystalline form; and that mixtures of cholesterine and bile pigments, dissolved in hot acetic acid, form, on cooling, homogeneous spheres, which gradually break up into smaller spheres, and then again into needles, which arrange themselves in a mixed laminar and crystalline structure around the centre of each sphere.—*Lancet*, Jan. 10, 1880.

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*The Relationship of Bright's Disease to other Diseases. The Primary Forms of Nephritis.*

In our preceding number (p. 164), we gave at some length the results of Prof. BAMBERGER'S researches into the connection between the secondary forms of Bright's disease and the diseases in which they originate.[1] We now propose to follow him in his investigation of the primary forms, and especially of their sequelæ. It will be remembered that the total number of primary and secondary cases of nephritis examined by Professor Bamberger was 2430, and of these 807 were primary—that is, cases either of parenchymatous (tubular) or of interstitial nephritis occurring in previously healthy subjects. Professor Bamberger remarks at the outset that the origin of the disease can only be referred in a minority of the cases to catching cold or getting wet; in the majority it cannot be explained even after the most careful clinical inquiry. The 807 primary cases included 67 acute, 357 chronic tubular, and 383 interstitial nephritis.

The sexes were represented by 445 males, or 55 per cent. of the whole number, and 362 females, but Professor Bamberger shows that the proportion of female to male cases is not accurately brought out by these figures. According to the ten years' mean the proportion of male to female patients of all kinds in the Allgemeines Krankenhaus at Vienna is as 63 to 37. Hence, if primary Bright's disease were equally common in both sexes, the numbers should be 508 males and 299 females; whereas, in fact, the preponderance is on the side of the females in the cases before us; and if the secondary forms associated with pregnancy and parturition were included, it would be still greater.

The ages of the primary cases for each decennial period are given in the following table. Professor Bamberger states that the majority of cases of acute nephritis (54 out of 67) occurred between ten and forty years, of chronic parenchymatous nephritis (214 out of 357) between thirty and sixty, and of granular contracted kidney (238 out of 383) between forty and seventy years.

| Decennial period. | No. of cases. | Males. | Females. |
|---|---|---|---|
| 1 | 1 | — | 1 |
| 2 | 50 | 33 | 17 |
| 3 | 121 | 64 | 57 |
| 4 | 149 | 93 | 56 |
| 5 | 156 | 94 | 62 |
| 6 | 152 | 78 | 74 |
| 7 | 115 | 57 | 58 |
| 8 | 55 | 22 | 33 |
| 9 | 8 | 4 | 4 |

The sequelæ, or the diseased conditions which complicate the later stages of the chronic forms of primary Bright's disease, offer more interest than the (at present) barren subject of the etiology of the latter. Beginning with affections of the brain, we find that cerebral hemorrhage occurred in 82 of the 807 cases, 52 of these being associated with granular kidneys. Two points are worth

[1] Volkmann's Sammlung Klinischer Vorträge, No. 173.

notice here: first, the great size of the extravasations in the apoplexy of Bright's disease; and, secondly, the frequency with which the renal mischief escapes detection *intra vitam*, so that apparently healthy individuals are suddenly and unexpectedly struck down by fatal hemorrhage. Encephalitis, with localized *foci* of inflammatory softening and meningitis, are both of rather rare occurrence. The latter, especially the tubercular form, is a much commoner feature in secondary than in primary Bright's disease. Pneumonia, on the other hand—to turn to diseases of the lungs—is very common, and 163 cases, or 22 per cent., were complicated with it. It generally is of the croupous lobar kind, and here again the elderly people whom it kills are often only found to be the subjects of granular kidney (interstitial nephritis) in the post-mortem theatre. Recent pleurisy, generally of a fibro-serous character, and uncomplicated with pneumonia or any other lung affection, occurred fifty-seven times, or in 7 per cent. of the cases. The other chief sequelæ of Bright's disease observed by Bamberger—excluding heart diseases, which will be presently considered by themselves—were peritonitis (22 cases, or 2.7 per cent.), dysentery, and extensive intestinal catarrh, particularly the former (109 cases, or 13.5 per cent.), and general dropsy (215 cases, or 26.6 per cent.). Bamberger considers the dysentery as very probably due to the transudation of urea into the bowel, and its conversion there into caustic ammonium carbonate, according to the theory of Treitz,[1] to which he refers.

Of all the pathological changes secondary to Bright's disease which the heart undergoes, none is, of course, so important as hypertrophy; fatty degeneration and pericarditis, generally serous, are, however, not so very rare, and the former was found in 97 cases, the latter in 99, 64 times as recent inflammation. Endocarditis and myocarditis are both so seldom met with as to need no further mention here. On the other hand, the frequency of cardiac hypertrophy and dilatation is most remarkable. Of Bamberger's 807 cases of primary Bright's disease, they occurred in 344, or 42.6 per cent. of the whole number. For the sake of brevity, we append a table of the different combinations of hypertrophy and dilatation which were associated with the three forms of nephritis adopted by Bamberger in his classification. It shows that the right side of the heart is *never*, the left side often, alone hypertrophied. That excentric hypertrophy of the whole heart is the most frequent condition (in 41 per cent.), and excentric hypertrophy of the left side (31 per cent.) the next in frequency, while simple hypertrophy of the whole heart and simple dilatation are rare.

| | Form of Bright's Disease. | | | |
|---|---|---|---|---|
| | Acute. | Chronic tubular. | Atrophic (granular). | Total. |
| Excentric hypertrophy of whole heart . | 3 | 51 | 88 | 142 |
| Excentric hypertrophy of left ventricle . | 4 | 38 | 65 | 107 |
| Simple hypertrophy of whole heart . | 2 | 3 | 6 | 11 |
| Simple hypertrophy of left ventricle . | 2 | 24 | 39 | 65 |
| Simple dilatation of whole heart . . | 4 | 5 | 7 | 16 |
| Simple dilatation of left ventricle . . | 0 | 1 | 2 | 3 |
| Total . . . | 15 | 122 | 207 | 344 |

But what a careful reader will at once notice in this table is that, as Bamberger says, "Hypertrophy of the heart occurs in every period of the disease, but with greater absolute as well as relative frequency the longer the disease lasts." Thus, 122, or more than a third of the 344 cases, were associated with parenchymatous, or tubular, nephritis; whereas it is generally taught and believed,

[1] "Ueber urämische Darmaffectionen" (Prager Vierteljahrschrift, 1859, Band iv.).

especially in England, that cardiac hypertrophy is the appanage of interstitial nephritis. Bamberger's position on this question is as follows: After studying it carefully for years, he finds that cardiac hypertrophy is met with in both forms of primary chronic Bright's disease, but decidedly more often in the interstitial form. Its relative frequency in the latter is, however, not sufficient for it to be made a point of distinction between tubular and interstitial nephritis; and he regards the difference between the two forms in this respect, not as the result of different pathological processes, but *of the longer or shorter duration of the renal affection.*

Before entering into a short discussion of Bamberger's theory of the actual cause of the hypertrophy, it may be interesting for a moment to illustrate by his figures the remarkable way in which certain of the complications of primary Bright's disease increase in frequency directly as the duration of the malady, while others, on the contrary, diminish in the later and reach their maximum in the middle period. The latter statement is true of pneumonia, pleurisy, dysentery, and general dropsy.

| Complication. | Percentages. | | |
|---|---|---|---|
| | Acute. | Chronic tubular. | Atrophic interstitial. |
| Fatty degeneration of the heart . . | 6.0 | 12.0 | 13.0 |
| Cerebral apoplexy . . . . . | 1.5 | 8.0 | 13.6 |
| Encephalitis . . . . . | 1.5 | 3.0 | 4.4 |
| Pericarditis . . . . . | 3.0 | 8.4 | 14.4 |
| Cardiac hypertrophy . . . . | 22.4 | 34.2 | 54.0 |
| Pneumonia . . . . . | 2.0 | 22.0 | 19.0 |
| Pleurisy . . . . . | 7.5 | 9.0 | 5.0 |
| Dysentery . . . . . | 12.0 | 16.0 | 12.0 |
| Dropsy . . . . . | 33.0 | 33.0 | 19.0 |

Without discussing this table in detail, we cannot help noticing the large percentage of cases of cardiac hypertrophy (nearly half) associated with the acute and chronic tubular forms of Bright's disease. It will be seen that Bamberger's statement that cardiac hypertrophy occurs in all stages of primary Bright's disease, but with greater relative frequency the more chronic the latter, is fully borne out.

And now we come to the question of the actual connection existing between the renal and cardiac affections, as to which Bamberger admits two possibilities: either the hypertrophy depends on the renal disease, or both are dependent on a third common factor. The possibility of the renal disease being secondary to the hypertrophy is of course excluded in dealing with "primary" Bright's disease. After discussing the various theories which, from Bright onwards, have been offered in explanation of the hypertrophy, Professor Bamberger comes to the conclusion that there is nothing else for it but to accept the first possibility just mentioned, and then to explain the changes in the heart on mechanical principles by the accommodation of the organ to increased demands on its propelling powers. At first sight this looks only like Traube's theory over again (increased work from destruction of renal structure, and impediment to excretion of water from the system); and in reality the two theories are not dissimilar. Bamberger, however, considers the renal process *per se* as only effective in so far as it interferes with the urinary function. He believes that the cardiac hypertrophy is the result of "the absolute increase in the general mass of the blood owing to the diminished excretion of water by the kidneys." The course of affairs, according to him, runs thus: Excess of water in the system raises the arterial blood pressure, the left ventricle dilates and hypertrophies, and throws

more blood into the veins; hence venous plethora, more work thrown on the right side of the heart, and dilatation and hypertrophy of the right ventricle. This theory, as Bamberger himself admits, is open, among others, to one principal objection —that while retention of water in the system is certainly of common occurrence in the more acute forms of primary Bright's disease, in many chronic cases, and especially in those of so-called contracted kidney (where cardiac hypertrophy is most pronounced), there is an apparent polyuria. To this, however, he replies that we know scarcely anything clinically of the early stages of contracted kidney; but we are almost certain, from the anatomical researches of Klebs, Buhl, and Thoma, that the progressive changes by which the urinary tubules are gradually surrounded by connective tissue, and their epithelial structures destroyed, must for a time at least diminish the excretion of water through the kidneys. It is during this period that Bamberger assumes the development of the cardiac hypertrophy. The polyuria, as we understand him, occurs later, when the collateral circulation in the kidney, which Thoma has especially described (*Virchow's Archiv*, Band 71), has established itself. A second and somewhat trifling objection to the theory is, that cases of anuria prolonged for one or two weeks have been recorded without the development of cardiac hypertrophy. Bamberger points out that not only must we know whether water was drunk, and in what quantity, during the anuric period, to form a judgment on these cases, but that the impaired state of the patient's nutrition must be remembered too. He regards them as difficultly explicable exceptions to the general rule. A third more important objection is, that retention of water ought to cause dilatation either of the whole vascular system, or at least of the arterial portion, which it has not been proved to do. The answer here is, first, that the subject has as yet been but little investigated; and, secondly, that it is very difficult to assign a mean circumference to the various portions of the aortic system, but that measurements which have been made (by Ewald) do show some increase in the circumference of the aorta under these circumstances. A final objection, that other diseases of the kidney which limit its secretion do not cause hypertrophy of the heart, is met by the answer that such diseases (cystic degeneration, multiple abscess, cancer, echinococcus, etc.) are not only very rare, but that they seldom fulfil the conditions necessary to the development of hypertrophy—sufficient duration of the disease, which must be bilateral, and which must not materially affect the volume of the blood, or greatly impair the general health. As a fact, cases in point have been reported by Cohnheim, Galabin, Stiller, and Bamberger himself. And what becomes, it may be asked, in Bamberger's hands, of the other theories of the origin of cardiac hypertrophy in renal disease? Traube's theory, as we have seen, he partly accepts and partly rejects.

Bright's original theory, that the heart enlarges owing to some change in the quality of the blood, as well as Senator's, in which urea is the substance that raises the arterial pressure in parenchymatous nephritis, and Ewald's,[1] in which a change in the blood, due to the renal disease, increases the resistance in the systemic capillaries, he rejects. He admits the possibility, but not the probability, of such a mode of origin of the cardiac hypertrophy. In the same way he refuses to admit Buhl's idea,[2] that the heart changes are due to previous inflammation, which occurs at the same time as, but independently of, the renal disease. Two facts he specially points out are fatal to Buhl's theory—first,

[1] Medical Times and Gazette, vol. ii. 1878, page 106. See Monthly Abstract, 1878, pp. 258 and 406.

[2] Ibid., vol. i. 1879, page 648. See Monthly Abstract, 1879, pp. 19 and 455.

that inflammatory affections of the heart are of late occurrence in Bright's disease; and, secondly, that Buhl himself found all traces of inflammation absent in 21 per cent. of his cases of cardiac hypertrophy and renal disease.

Lastly, Bamberger rejects the essentially identical theories of Dr. G. Johnson and of Sir William Gull and Dr. Sutton, which refer the cardiac hypertrophy to a general affection of the small arteries and capillaries either in the form of muscular hypertrophy or of "hyaline-fibroid" degeneration. He admits that Dr. Johnson is right in describing muscular hypertrophy of the systemic arterioles with diminution of their calibre, but he denies that it precedes the cardiac hypertrophy, or that there is any proof that it does so, and he ascribes it to the increased arterial pressure which is the result of the heart-change. He also points out that these theories fail to explain the hypertrophy of the *right* side of the heart. It should be added that Bamberger is of Dr. Johnson's opinion, that arterio-capillary fibrosis, or "hyaline-fibroid" degeneration, is a product of microscopical preparation. In conclusion, if we may be allowed to express an opinion on the work of so eminent a man as Bamberger, we should be inclined to say that even *his* theory scarcely answers to the requirements of a good theory—namely, that it shall explain all the facts naturally and without being strained. It is, however, a good theory in this respect: that it suggests several points for future inquiry, which may be very fruitful in directing us to the true *rationale* of the connection between cardiac hypertrophy and renal disease. Might we suggest that we know too little about the reason for the altered function of the skin in chronic Bright's disease? Why do patients thus affected cease to perspire, and why does their skin became so dry and unaffected by the ordinary stimulants of the sweat-glands? In diabetes we can understand that the dryness is caused by the drain of water from the body; but in Bright's disease, where more or less retention of water is the rule, the explanation is difficult. Possibly, whatever the cause, one of the factors in the production of cardiac hypertrophy is this impaired function of the skin; in any case the phenomenon is of great interest, and deserves more attention than it has yet received.—*Med. Times and Gazette*, Jan. 10, 1880.

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## Surgery.

### *Recent Researches in Pyæmia and Septicæmia.*

A most valuable scientific summary of clinical reports and pathological research will be found in the new volume of the *Transactions of the Pathological Society*. We refer to the report of the Committee appointed by the Society to investigate the nature and causes of pyæmia, septicæmia, and purulent infection. It consists of statistics collected from 1869 to 1878, in the London hospitals; of one hundred and fifty-six detailed reports of cases, arranged according to their clinical and pathological features, and compared with diseases produced artificially in animals; and of observations on the blood, urine, and microscopical appearances of organs in typical cases.

The greatest mortality from pyæmia, during these ten years, was in 1874–5. The early spring of 1874 was very warm, but the month of May very cold; severe winter set in on November 20th; and the rain-fall for the whole year was much below the average. The last half of the succeeding year was cold and damp. The monthly death-rate is greatest, according to this report, in February and March. The Committee invite inquiry as to the significance of these meteorological facts. The undue closing of ward windows during east

winds, or at a sudden fall of the temperature after a run of fine early spring weather, or the great space of time from the last annual hospital cleaning, may explain the meaning of these two bad years and months.

The difficulty of defining the term "septicæmia" is dwelt upon, and, after quotations of the definitions of Burdon Sanderson, Billroth, and other authorities, reference is made to Koch's well-known experiments, which had the practical result of distinguishing a "septic or putrid examination" from a "septic or putrid infection." A heavy dose of putrid fluid, injected under the skin of a mouse, killed it in a few hours by pure chemical poisoning, leaving no signs of inflammation; nor could any bacteria be found in the animal's blood, which fluid produced no ill effects when injected into another mouse. But, when a small amount of the same putrid matter was injected, the mouse in some instances recovered; but in others, after about twenty-four hours of immunity, fatal symptoms of acute blood-poisoning set in; the blood, which was charged with bacilli, became highly infective to other animals, and œdema was found around the injection-puncture. The first condition—septic intoxication—to occur in man, must presume the admission of a considerable amount of putrid blood; so that it may be expected only after operations involving large irregular wounds, or serous and synovial cavities which have rapid powers of absorption, or in a case of acute abscess opened externally by an insufficient aperture, leaving septic matter pent up at a sufficiently high degree of pressure to allow absorption through the granulation-tissue of the walls. In twenty-eight cases of fatal blood-poisoning, without formation of secondary centres of inflammation, only two seem entitled to be classed as "septic intoxication," but on evidence admitted not to be conclusive. It is much to be regretted that cases of ovariotomy were not recorded, as, after that operation, the conditions of septic intoxication are all present; and hence that complication often appears, or at least did so before drainage and antiseptic precautions came into vogue.

The clinical distinction of septic infection from septic intoxication is beset with difficulties, though the former must be the more common disease. It may be suspected to exist in its pure form when a wound is too small to allow the rapid entrance of a dose of poison sufficient to produce septic intoxication. A large wound may, of course, involve septic infection; but, as it might be then associated with septic intoxication, the former could not, in such a case, be studied in its purest form. Evidence of infection from patient to patient is conclusive as a distinction, when it exists, as it cannot occur in pure septic intoxication; but absence of such evidence does not prove the contrary. Out of twenty-eight cases of blood-poisoning, in which death occurred without the formation of secondary abscesses, the Committee could only find two arising from wounds too small to admit enough septic matter to produce direct septic intoxication. Contrary to Billroth's assertion, rigors were frequently observed in the reported cases of pure septicæmia. In many instances, the period of invasion was very late, yet no secondary centres of inflammation could be detected after death. In other words, septicæmia is not a mere early form of pyæmia.

As to pyæmia itself, the Committee prefer to understand the term in the sense in which it is defined by the College of Physicians. The effects of injection of pus experimentally are described, according to the experience of various authorities. Inoculation of an animal with the blood or discharges from a case of pyæmia in man has never resulted in the precise reproduction of that disease; but, on referring to Appendix III. of this report, we find a record of Birch-Hirschfeld's experiments, which show that the injection of healthy pus produces nothing beyond transient fever; whilst pus containing micrococci caused severe fever, suppuration around the seat of injection, and, in a few cases, metastatic abscesses.

The Committee have classified cases of pyæmia in groups, according to the presence, association, or absence of each and of all of the well-known pathological features of the complaint—such as visceral abscesses, thrombosis, embolism, etc. Separate classes are also made for cases of pyæmic symptoms following acute necrosis and infective endocarditis; also for "spontaneous" septic disease; and for the "infective myositis" of Nicaise, of which one case is given in the statistics.

The blood was minutely examined in eighteen cases. The white corpuscles were nearly always greatly in excess, both absolutely and relatively, to the red, without the changes in their characters as seen in leucocythæmia; the amœboid movements remained natural, or were unusually active. The proportional increase ranged from one white among one hundred red corpuscles to one white among eighteen red. As to the absolute increase, whereas, under the usual method of inspection and calculation, 2.5 white corpuscles were found on the field when healthy blood was examined, the lowest average on the field in pyæmia was twelve, and more than seventy in cases sometimes seen. The evident diminution of red cells must be taken into account.

The examination of the blood for bacteria yielded very different results in many cases. As to the statement that micrococci may be found in large quantities in the white corpuscles, the Committee could not recognize such a condition in any case; the difficulty of distinguishing these germs in the liquor sanguinis is dwelt upon. Half-a-dozen varieties of bacteria were found; and, in conclusion, so far as these observations go, it is shown that "no one form of bacteria is peculiar to any one kind of disease."

No special characteristic could be found distinguishing the urine in cases of blood-poisoning from that of any acute form of pyrexia. It was generally acid, very soon becoming ammoniacal, and was about normal in quantity in the majority of cases. Albumen, never in excessive psoportions, was nearly always present; urea was excreted about one-fourth above the normal average; and uric acid in double the amount of the healthy excretion. The inorganic constituents, especially the earthy phosphates, were diminished in quantity. Although the red corpuscles are diminished or destroyed, no potash is thereby set free in pyæmic urine; for that base is found below its normal proportions. Possibly, it is remarked, the increase of white corpuscles may account for the potash not being present in greater quantity in the urine after the destruction of the red. In scurvy, there is likewise diminution of the number of red corpuscles and diminished excretion of potash in the urine, that base being possibly retained in the blood to maintain its alkalinity, lessened by the withdrawal of the alkaline carbonates of the food.

On microscopical examination of different organs, micrococci were found in all, or at least in some, of the viscera, and their distribution is very unequal. They were nearly all in the bloodvessels, completely plugging capillaries; in masses which sometimes produced varicosities, or even ruptured the vessels, and extended into the contiguous tissues. Great pathological changes were often found in structures infected with micrococci, yet sometimes the immediate surroundings of these organisms presented little, if any, alteration from the normal condition. In larger vessels, the micrococci may line the walls as an even layer, or project from them as papillæ, which occasionally break off, and in all cases may involve coagulation of the blood passing by them. Bacteria were found in three cases, but in two there was evidence of their origin being *post mortem;* in the third, a case of acute infective myositis, rod-shaped bacteria were present in large areas within the wall of the left ventricle, within and around the vessels, and between the muscular bundles, but hardly any could be found within the sarcolemma.

This appearance is represented in one of Dr. Greenfield's beautiful microscopic drawings, which, together with an equally fine series illustrating Mr. McCarthy's observations, accompany the report.

The obliterative endoarteritis was very frequent in arterioles in all the tissues; thrombi were commonly found in the vesicles. The walls of veins containing thrombi were often surrounded by minute abscesses, and partially destroyed, so as to be indistinguishable, except by the proximity of the corresponding arterioles found alongside the abscesses. The connective tissue had in all organs undergone changes—such as hypertrophy, increase of nuclei, swelling of the fibrous bundles or miliary abscesses.

Micrococci were seen in the liver, but not so frequently as in the kidney. They were mostly in the capillaries. Owing probably to embolism, certain acini were found to be in a very degenerate condition; whilst others, near them, remained almost unchanged. In all cases there was some exudation in the portal spaces. The hepatic cells showed signs of more or less fatty degeneration, and in one instance there was a remarkable segmentation of their protoplasm. In the kidney, micrococci were almost constant; their favourite seat was in the capillaries surrounding the straight tubules in the upper part of the Malpighian pyramids. The glomeruli undergo great changes, described very minutely in the report, together with some original observations on peculiarities in the epithelial lining of the uriniferous tubules. Micrococci were found in the substance of the valves of the heart, and in vegetations from those valves, in two cases of ulcerative endocarditis. In the lung, thrombi, plugging the veins and filled with micrococci, were observed; and in one instance, the wall of the vessel being ruptured, blood had become extravasated with micrococci into surrounding structures.

To this report are added two valuable statistical appendices, and two, consisting of "notes," or rather of records *in extenso*, of the microscopical examination of the blood during life, and the pathological histology of blood-poisoning. Drs. Ralfe and Greenfield, Messrs. Beck and McCarthy, deserve the thanks of the profession for having brought together such a mass of valuable facts in relation to one of the gravest classes of diseases with which the surgeon has to deal. We must continue to sympathize with these gentlemen, as we have done before, in lamenting, as they do, the absence of a uniform methodical system of registration throughout the various metropolitan hospitals. Until this is effected, the profession will not be perfectly free from Bacon's reproach. "The direction of future judgments," as he would have said, is not promoted by the neglect, at some institutions, of recording details which, at others, are carefully saved from oblivion.—*British Medical Journal*, Jan. 24, 1880.

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### *Retinal Changes in Kidney Disease.*

The frequency with which retinal changes are found in kidney disease has been variously stated by different authorities. Earlier statistics were based on affection of vision, and the frequency assigned was altogether too low, while some ophthalmic surgeons, seeing cases only in which sight was impaired, have thought that retinal changes were almost invariable. WAGNER found albuminuric retinitis in 9 per cent. of the cases, GALEZOWSKI in 33 per cent., and LAUDOUZY in almost every case. It is, however, well known that the frequency with which retinal changes occur varies much in different forms of kidney disease, and it is, therefore, desirable that in observations which are made, the forms of kidney disease should be distinguished. This has been done by Mr. EALES, of Birmingham, who in an interesting communication to the current number of the *Birmingham Medical Review*, has described an investigation of the state of the retina in

one hundred cases of granular kidney, the primary object being to determine the percentage of cases in which retinal changes are present. For the diagnosis of the renal disease in every case Dr. Saundby was responsible. The results contained confirm nearly the statement of Galezowski, alterations being found in one-third of the cases. They confirm also the fallacy of taking sight as the test of retinal integrity. In forty-six cases inquiries were made as to the state of vision before atropine was instilled; in twenty-eight of these complaint of bad sight was made, but in only six (that is, a little more than one in five) could any obvious abnormal condition be discovered in the retina; while in eighteen persons, who stated that their sight was good, retinal changes, such as specks, were found in five. The frequency with which considerable retinal changes do not materially impair sight is shown by six cases in which vision was carefully ascertained by test-types and lenses, and found, even with dilated pupils, to be nearly normal, and yet in all considerable changes were found in the retina.

The number of cases in which some abnormal change was found in the retina was twenty-eight, in twelve of which the changes existed in both eyes, while in sixteen they existed in one eye only. Observers have usually described the changes as bilateral, and Mr. Eales thinks that his observations can only be reconciled with those of others by supposing that the affection often attacks one eye before the other, and that it gets well in one eye before the other. In the twelve cases in which changes were found in both eyes, four had diffuse retinitis, with "fibrinous" patches and œdema, and also whitish glistening patches; one had diffuse retinitis in one eye, with only one hemorrhage in the other eye; five had many whitish round patches, "of the atrophic kind," and two had only similar small patches. Of the sixteen cases in which changes were found only in one eye, in six several white round patches were found, in five one or two spots only; in one there was a single hemorrhage near the disk; in two a few black specks were found, associated in one case with white specks, the black being in the centre of the white speck. In addition to these twenty-eight cases of retinal change, three others presented slight changes in the papilla, commencing or subsiding neuritis. In fourteen cases opacity of the lens existed, double and incipient in eleven cases, single in three cases. The presence of these alterations bore some relation to the amount of albumen in the urine, and one in every two patients with constipated bowels presented retinal changes, while such changes were found only in one in every six with open bowels.—*Lancet*, Jan. 31, 1880.

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*On the Surgical Treatment of Intestinal Obstruction.*

A recent discussion at the Society of Surgery of Paris between MM. Terrier, Duplay, Polaillon, Tillaux, L. Le Fort, Ledentu, and Trélat, turned upon the subject of intestinal occlusion and its treatment. This subject has been so recently discussed by English surgeons, that it will be a matter of interest to summarize the recent opinions of the younger school of French surgeons. M. TILLAUX observed that the surgery of the abdomen is undergoing, at the present time, transformation. "I am convinced," he said, "that if M. Nélaton had taken part in the present movement in surgery, if he had seen the abdomen opened with the security that now belongs to this operation, he would have accepted gastrotomy. Enterotomy" (formation of artificial anus), he added, "is not a rational operation." Up to this time, enterotomy has been considered as a true benefaction. It was the only admissible operation against internal strangulation. However, the idea of performing laparotomy, *i. e.*, largely opening the abdomen, and of searching for the seat of strangulation, is not new. M. Le Fort, with his well-known erudition, reports cases of Bonnet (1700), of

Nuck, and Dupuytren in 1817, who did a gastrotomy, or rather a laparotomy, for intestinal strangulation on a patient in the wards of Récamier. Since then, this operation has been performed a great number of times. Adelmann, in 1863, reported thirty-three operations of laparotomy, practised for various causes. There were fourteen cures, and nineteen deaths. Ashhurst, in 1874, gave more complete statistics, and classed amongst them cases of invagination. Thirteen operations gave five cures, and eight deaths. To these cases, M. Le Fort adds those of Miller, who cured a child of seven months by laparotomy; also those of Hutchinson and of Fagge.

In a thesis supported before the faculty in 1872, Dr. Delaporte, a pupil of M. Boinet, affirmed that laparotomy was far from being such a dangerous operation as had been hitherto alleged, and he reported fourteen cases giving a proportion of eight cures, that is, 57 per cent. M. Bulteau has collected, in a recent *Thèse de Paris*, 92 cases of intestinal occlusion, in which laparotomy had been practised. An analysis of these 92 cases gives the following results. 1. In ninety-two operations of laparotomy, there were thirty-three cures and sixty-nine deaths, that is, 36 per cent. of cures. 2. In twenty-one operations of laparotomy in cases of invagination, there were eight cures and thirteen deaths, or 38 per cent. of cures. 3. In seventy-one operations of laparotomy in all kinds of occlusion other than invagination, there were twenty-five cures and forty-six deaths, that is, 35 per cent. of cures. These statistics have only a relative value. Those of M. Bulteau are incontestably in favour of laparotomy. Equally satisfactory statistics are required in the case of enterotomy and artificial anus. In six patients, Nélaton had two successes. M. Ledentu says, that in four operations he had three deaths, M. Polaillon has practised enterotomy five times on the method of Littré. All cases proved rapidly fatal, with the exception of one which lived fifteen days. In six cases, M. Tillaux had only one success, in a cancerous patient. Statistics are entirely wanting to enable us to compare the respective value of enterotomy and laparotomy. However, it seems from the interesting discussion which has just taken place in the Société de Chirurgie, that all surgeons are agreed that, in certain cases, laparotomy is to be preferred.

These are the reasons which M. Le Fort gives. "We do not possess, or at least I do not know of, statistics of operations of colotomy; but, from what I have seen of my own practice and in the practice of my masters and my colleagues, I think that cure by colotomy is exceedingly rare. I have lost all my own cases; and M. Verneuil, whose practice is more extensive than mine, tells me that he has not a single case of cure. Further, enterotomy, though it may relieve the patient in allowing evacuation by the upper end of the intestine, does nothing to cure the disease. The structure, or invagination, or the volvulus, still exists; and if the intestine is much strangulated—and if, therefore, its vitality is much compromised—one may be quite certain that peritonitis will ultimately prove fatal to the patient. Enterotomy causes the intestine to be wounded, which laparotomy avoids; and, considering the susceptibility of the visceral peritoneum, particularly the intestinal peritoneum, which is extremely vascular, to peritonitis, it being much more irritable than the parietal peritoneum, the fear of an error in diagnosis should make one hesitate to perform enterotomy; but if the surgeon has, at the moment of operation, reason to regret having made an error in diagnosis, this error is irreparable if enterotomy is performed. Laparotomy, on the other hand, permits one to recognize the error, and thereby prevents useless interference."

M. Ledentu said: "The real advantage of laparotomy is, that if it succeeds the course of the fecal matters is re-established, whilst enterotomy exposes the patient to a persistent and most painful infirmity. If, however, after this

latter operation, the intestine is reopened, how many times can one count upon this result? No one can say. To enable one to interfere at the right moment, laparotomy is a resource much more rational, and perhaps less perilous than the purgatives with which one ordinarily tries to overcome internal strangulation."

M. Lucas-Championnière went further. He said: "The operation is excellent; we have only to discuss the right opportunity for laparotomy; we must reject the operations which penetrate the lateral walls through the muscles, as much the more complicated; and though laparotomy may present serious difficulties they are not insurmountable by the surgeon, particularly the escape of the distended intestinal loops."

In reply to the question in what cases laparotomy should be employed, M. Bulteau replies as follows. 1. In all cases of acute intestinal occlusion, as soon as the ordinary medical means employed have failed, gastrotomy should be practised. The nearer gastrotomy is to the commencement of the accidents, the greater is the chance of success. 2. In chronic intestinal occlusion, sometimes gastrotomy, sometimes enterotomy, and sometimes lumbar colotomy should be employed. (*a*) Gastrotomy should be practised in cases of chronic intestinal invagination, when insufflations of air, injections of water, and the application of electricity have not been able to reduce the intestine. To reduce invagination, which is often difficult, the method of Hutchinson should be employed. According to him, "the lower part of the invagination should always first be sought for, and the reduction attempted by squeezing the intestine, or by pulling at the invaginating portion rather than by seeking to directly extricate the invaginated loop." (*b*) In occlusion of the large intestine, if the precise seat of the obstacle be not known, cæcal enterotomy should be performed. (*c*) If the examination of the patient allow one to determine that the seat of occlusion is in the course of the sigmoid flexure, and of the rectum, Littré's operation of enterotomy should be practised in the left groin, or better still, lumbar colotomy of Amussat. (*d*) In all the other varieties of chronic occlusion of the intestines, Nélaton's enterotomy should be performed in the right iliac fossa.

What method of laparotomy should be employed? Methods vary. M. Parisse, of Lille, makes his incision in the right iliac fossa for two reasons; first, because it is by visual examination that one has the best chance of meeting with the strangulation; secondly, because one finds in the right iliac fossa a landmark which is certain and easy of recognition—the cæcum. Other surgeons make a very large incision in the linea alba. By this method, one is not at all interfered with by intestines, even when distended, in the search for the seat of obstruction. "One is struck," said M. Lucas-Championnière, "with the facility with which the search is made. In both operations, it is the cæcum by which the surgeon may guide himself, in the sometimes difficult search for the strangulated place. If the cæcum and the large intestine be collapsed, one will be sure that the obstacle is seated in the small intestine. On the contrary, if the cæcum and a part of the large intestine be distended by gas and fecal matters, one may be sure that the obstacle is seated at some point of the large intestine." "The surgeon may, therefore," says M. Bulteau, "determine the seat of the obstacle as soon as the belly is opened, without, so to speak, touching the intestinal mass." If the cæcum and large intestine be collapsed, the surgeon should examine the right iliac fossa and the inferior extremity of the ileum, starting from the point at which this intestine opens into the cæcum; he will arrive inevitably and rapidly at the seat of the obstacle by continuing his search along the small intestine. The statistics of intestinal occlusion show in a palpable manner that, in the cases of occlusion of the small intestine, the obstacle is seated almost always in the right iliac fossa, and almost always in the lower part of the ileum. The

surgeon, by examining first the right iliac fossa, has, therefore, a great chance of rapidly meeting with the obstacle which he wishes to remove. If the cæcum be distended by gas, the surgeon must then examine the ascending colon, the transverse colon, etc. It is in the large intestine, in fact, that the obstacle is seated, and his examination should be limited to that organ.

The seat of the obstacle to the passage of fecal matters once found, the state of the intestine must be examined. When the intestine is only congested and appears healthy, it may be left in the abdominal cavity, after the strangulation has been removed and the wound sutured; on the other hand, if the intestine be perforated or gangrenous at the point at which the strangulation is seated, an artificial anus should be established. The success of the operation depends largely on the rigorous cleanliness observed by the surgeon. The antiseptic method should be employed in all its exactness, and the same precautions of bandaging as those for ovariotomy.

M. Lucas-Championnière insists greatly on the necessity of opening the belly largely in the median line. The incision in the right iliac fossa, in fact, is too restricted to permit the surgeon to explore sufficiently the cavity of the peritoneum and to remove the obstacle to the course of the intestinal contents. The incision in the linea alba has the advantage of permitting the recognition of the obstacle, whatever be its seat. For the rest, it is well known how difficult it is to determine beforehand the precise seat of the obstruction. [See also Duplay on Enterotomy and Laparotomy, *Archives Générales*, Dec. 1879.]—*London Med. Record*, Jan. 15, 1880.

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*The Value of Parallelism in the Treatment of Disease and Injuries of the Hip.*

At a late meeting of the Medical Society of London (*Lancet*, Jan. 24, 1880), Mr. Thomas Bryant read a paper on the value of parallelism of the lower extremities in the treatment of hip-disease and hip-injuries, with the best means of maintaining it. The author first pointed out how it was that, in most of the deformities of the lower extremity following a natural recovery with a stiff joint after hip disease or injury, the deformity was due to a want of parallelism of the lower extremities, and then passed on to demonstrate, by means of a simple instrument he had constructed for the purpose, that, as a rule, in such cases the deformity is produced by the *ad*duction of the affected limb. He conclusively demonstrated that when the hip-joint is fixed in an *ad*ducted position, and the patient attempts, for walking purposes, to bring the limb vertically downwards, the pelvis on the affected side is tilted upwards, and on the sound side downwards, with the result of increasing the shortening of the affected limb. On the other hand, when the anchylosed limb is *ab*ducted and the foot is brought down for the purpose of progression, the pelvic line on the diseased side is lowered, and the affected limb is merely apparently elongated; apparent lengthening of the diseased limb in hip disease meaning its *ab*duction, and apparent shortening its *ad*duction. Under these circumstances it is clear that only by maintaining parallelism of the two limbs in such cases can deformity be prevented. For this purpose the use of the double splint, which he has had slowly built up under his eye at Guy's, was strongly recommended, for he maintained (1) that it is far more comfortable to the wearer than any other he had used or seen used; (2) that it most thoroughly immobilizes the limb to which it is applied; (3) it keeps up well-regulated, steady, and persistent elastic extension; and (4) maintains absolute parallelism of the lower extremities. In children with acute hip disease, the splint, when well applied, so securely guards against joint movement that the nurse may turn the child over on to the sound side for the purposes of cleanliness,

and lift him for the performance of the natural functions, with freedom and confidence. In cases of fracture of the neck of the femur or shaft, the same principle is applicable and the same splint of value; in proof of which the author stated that thirty consecutive cases of fracture of the neck of the thigh bone, occurring in patients averaging seventy-four years of age, left his wards with useful limbs; and that out of forty consecutive cases of fracture of the shaft of the femur, in only four, or one in ten, was there any important shortening, such as an inch. In twenty of the remainder there was no shortening, and in eighteen there was less than half an inch.

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*Subastragaloid Dislocation.*

At a late meeting of the Clinical Society of London Mr. PICK read notes of a case of subastragaloid dislocation. A gentleman fell off the roof of an outhouse, and caught his foot in falling. When seen, the foot was so twisted that the sole looked outwards, whilst the astragalus formed a marked prominence below the inner malleolus beneath the tightly-stretched skin; the os calcis projected beyond the outer malleolus, and the lower end of the fibula was prominent. It seemed that the os calcis and scaphoid had been displaced outwards and backwards. Attempts at reduction under chloroform, and after division of the tendo Achillis and peronei tendons, failed. Much force was used, the foot was brought more into the natural position, but still the relative positions of the os calcis and astragalus remained unaltered. Subcutaneous injection of morphia, and chloroform inhalations were had recourse to for the relief of the severe pain experienced. The next day, in consultation with Sir J. Paget, Mr. P. Hewett, Mr. Henry Lee, and Mr. Gage Brown, another attempt at reduction was made under chloroform. At Mr. Lee's suggestion a bandage was placed round the forepart of the foot, and by this means Mr. Lee made extension forwards, whilst Mr. Pick made lateral movements. The attempt was successful, and, with the exception of a superficial sloughing of skin, where it had been so much stretched, the case made an uninterrupted recovery. It is now three years since the accident, and, beyond a slight limp, there is nothing perceptible in the limb. The contour of the foot is perfect. Mr. Pick then described what he thought to have been the condition in this case. He had seen similar dislocations reduced by slight extension, but in the majority of cases reduction was very difficult, owing partly to the tibial tendons being caught behind the astragalus, but mainly to the interlocking of the bones. The foot is thus displaced backwards, and also laterally. Mr. Hancock had shown the value of forward extension, with the bones of the leg fixed, in the reduction of such dislocations.

Mr. BRYANT said this was an interesting example of a rare form of injury, and he agreed with the author as to the great difficulty of reduction in such cases. In a case he met with some time ago he concluded that the difficulty was due to the cause pointed out by Mr. Pick—viz., that the posterior edge of the astragalus was caught between the articular facets of the os calcis. It was not until he read Mr. MacCormac's paper in St. Thomas's Hospital Reports that he saw these cases of so-called "dislocation of the astragalus" in their true light. Mr. Lee's ingenious mode of reduction favoured the anatomical explanation given.

Mr. H. LEE pointed out that in literature, especially French, such dislocation was regarded as a very formidable affair, calling for amputation or excision, or the retention of a useless foot. The whole of the foot was carried outwards; the sole was turned directly outwards. He had thought much about the reasons of failure on the first attempt at reduction, and, taking this with other cases, he concluded that, with the exception of Mr. Hancock's case, the failure lay in the fact that extension had not been made in the right direction. It had been made

parallel to the axis of the limb; but as there were two displacements to deal with, viz., that of the os calcis from under the astragalus, and that of the scaphoid also, he concluded that traction of the scaphoid *forwards*, and not downwards, was the right course to pursue.—*Lancet*, Jan. 31, 1880.

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*Hyperostoses Consecutive on Obstinate Ulcers of the Leg.*

Dr. PAUL RECLUS publishes a careful study (*Progrès Médical*, December 20, 1879) based on the observation of cases. His conclusions are these. 1. Ulcers of the leg may give rise to divers forms of osteo-periostitis in the subjacent bones. Exceptionally destructive, the inflammation most frequently determines increase of volume; the bone is then light and spongy, covered with osteophytes. In very rare cases, the tissue of the diaphysis is hard and eburnated, the medullary canal obliterated or narrowed. 2. When the epiphyses are not yet joined, the bone increases in length, and may exceed its fellow by from 2 to 3 centimetres (¾ to 1¼ inch). The osteophytes are more abundant. A vertical osseous diathesis may manifest itself, which shows itself by the ossification of the aponeuroses of the ligaments of the vascular and nervous sheaths; in a word, of all the fibrous tissues of the leg. 3. From the centre of the ulcer sometimes arise prominent exostoses. These have been taken for syphilitic lesions, but the consistence of the tumour and specific treatment will establish the diagnosis. 4. Osteo-periostitis is a grave complication. It is inimical to the cicatrization of ulcers, compromises the nutrition of the limb, and very often renders amputation necessary.—*Lond. Med. Record*, Feb. 15, 1880.

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## Midwifery and Gynæcology.

*The Kidney Diseases of Pregnancy.*

Dr. R. MÖRICKE (*Zeitschrift für Gebertshülfe und Gynäkologie*, Bd. v. Heft 1) considers that venous stasis is the beginning, the chief etiological fact, of most cases of nephritis during pregnancy; but he does not deny that other causes of this disease may act during pregnancy as well as at any other time. He has made careful and repeated examinations of the urine of pregnant women, before and after labour, taking especial note of the presence and amount (as the most trustworthy signs of kidney disease) of albumen and of casts. He finds that as a rule the urine passed before delivery, even though it contain albumen, yet shows few, if any, casts. The first urine passed after delivery contains, on the contrary, casts extraordinary in size and number. The author thinks that a sudden change in the circulation is the only agency which could produce so rapid and great an effect. The albumen and casts disappear surprisingly soon, being sometimes completely gone on the second day. Out of twenty pregnant women whose urine the author examined, albumen was present in one; and out of one hundred women in labour, thirty-seven had albuminous urine, and in thirteen there were casts. In support of his view, that the kidney affection is due to venous congestion, the author adduces the facts that albuminuria is disproportionately frequent in first pregnancies and in twin pregnancies, and that it is usually only met with in the later months; that it rapidly disappears when the uterus is emptied; that in pregnancy there is obstruction to the return of venous blood, shown by the occurrence of varicose veins and œdema of the legs; that it is shown by experiments on animals, that such venous congestion is adequate to produce albuminuria, and also by its occurrence in diseases such as valvular incompetence, emphysema, etc., in which there is obstruction to the venous circu-

lation; and he considers that the peculiar condition of the blood in pregnancy is such as to favour the passing of the changes due to venous congestion into those of inflammation. To the objection raised, that large tumours are found in the abdomen without albuminuria, the author replies, that albuminuria is often met with in such cases. To the objection that it is impossible for the pregnant uterus to press on the renal veins, he answers that it is not necessary for it to press on them directly; its bulk raises the intra-abdominal pressure, and thus hinders the return of blood. The argument that albuminuria is sometimes met with in the early months of pregnancy, the author meets by stating that he does not assert venous congestion to be the only cause. The theory that the liability to nephritis is due to increased renal function, he rejects on the grounds that there is no evidence of such liability where one kidney is absent, or has been rendered useless, and extra work is thrown on the other; nor is there in diabetes. The view of Spiegelberg, that nephritis in pregnancy is due to a reflex action on the vaso-motor nerves of the kidney, Dr. Möricke looks upon as a pure hypothesis.—*Obstetrical Journal of Great Britain*, Jan. 15, 1880.

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*Puerperal Malarial Fever.*

At a late meeting of the Medical Society of the County of New York (*Med. Record*, Feb. 7, 1880) Dr. FORDYCE BARKER read a paper on the above subject, of which the following is an abstract: Although the title of the paper has not yet been added to the nomenclature of diseases in medical literature, it is so descriptive of the etiology, pathology, and clinical phenomena of a class of affections which of late years had been so frequently met with in puerperal women in New York and its vicinity, that he employed it. The author of the paper then presented the character of the disease, its pathology, its differential diagnosis, and its treatment. By the kindness of medical gentlemen who had furnished him with complete notes of cases which he had seen with them in consultation, and from those in his own private practice, he had the records of seventeen cases, and of these he gave a summary of the aggregate results and the conclusions to which he had arrived.

Puerperal malarial fever might make its invasion at any period following parturition, until the physiological changes which constitute puerperal convalescence were completed. The earliest development occurred in less than twenty-four hours after entirely normal parturition. The history of a case which occurred in the practice of Dr. Howard Pinkney was then given. The latest period of invasion was in one of his own patients, who had a normal labour in every respect, and her convalescence was so complete that he had begun to make weekly visits only. The details of the case were reported. The most prominent *symptoms* were chills, sometimes very slight, a temperature higher by one or two degrees, frequently, than was found in the beginning of any other puerperal disease, rapid pulse, greater prostration than was usual with other diseases during this period. After such an explosion, there was a remarkable remission on the following day, but the alarming symptoms returned after one, two, or three days, yet usually less severe, only typical cases, presented such a succession of phenomena. Dr. Barker was disposed to believe that in the majority of cases the patient, three or four days after the explosion, finds herself suffering from a general sense of malaise, more or less of pain in the back, head, and bones, thirst, loss of appetite, insomnia; and when the disease was developed the chills were less severe, the pulse less rapid, the temperature not so high, and the remissions less marked; and he was also of the opinion that in such cases the disease was more persistent and responded less readily to treatment. In puerperal malarial fever a fall of temperature three or four degrees was always attended with a corresponding decline of

other symptoms, which is not the fact in septicæmia, and the latter is rarely accompanied with pain in the head, back, and limbs. He thought that a competent and intelligent observer would not be likely to mistake the affection, when developed six or eight days after confinement, for pyæmia, even though it was developed late during the puerperal period. Puerperal fever usually appeared between the first and the third day after delivery, and very rarely after the fifth day, while the chills were not recurrent, nor were there marked remissions of the symptoms. Malarial fever may be developed during the progress of any of the local phlegmasia, or may be complicated by them. In five cases, secondary hemorrhage occurred after the twelfth day, apparently as the result of the malarial fever. One of these has already been reported by Dr. H. T. Hanks. In one case, three days after the hemorrhage, purpura was developed, and there was some oozing of blood from the nasal and buccal mucous membranes. In four of the cases seen in consultation the chief reason why he was called was that the patient was supposed to have puerperal mania. The details of a case seen in consultation with Dr. Barker, of Jamaica, L. I., were then given. But one of the seventeen cases had terminated fatally, and that was in the practice of Dr. William H. Hall. The patient died on the forty-seventh day after confinement. The history of the case was given. The author of the paper did not dwell upon the treatment, for the reason that the treatment of malarial fever is so well settled, and every physician of intelligence and sound practical sense appreciates the necessity for and the kind of auxiliary measures which the symptoms peculiar to each case may require. He had found Warburg's tincture much more effective and speedy in producing the results desired than the largest doses of quinia. His method was to give it in half-ounce doses every four hours until the fever had entirely abated, and then continuing it in gradually diminishing doses until convalescence is perfectly established. On the least threatening of a recurrence a full dose should at once be administered. The tolerance of quinia in these cases is very remarkable. One patient received 80 grains hypodermically within twenty-four hours, and yet no symptom of cinchonism was produced. When given by the mouth it is usually combined with bromide of potassium, to prevent or modify the cinchonism.

Dr. S. S. JONES remarked that it had been his fortune to see a great deal of malarial fever in puerperal women within the last two or three years, and some of the cases had been exceedingly persistent and difficult to treat. There was one feature not noticed by the author of the paper, and that was the fact that septicæmia can occur after the onset of malarial fever in the puerperal condition, and that it may be the occasion of elevation of temperature without remission for several days, thus accounting for the fact that quinia does not produce its effect as quick as it usually does in pure malarial fever. It had been his habit to give, living as he did in a malarious district, a dose of quinia on the day following the confinement. In those cases in which fever does not subside after the exhibition of a full dose of quinia, it had been his habit to cleanse the uterine cavity, and in one or two cases he thought he had almost demonstrated that the cleansing of the uterine cavity was the turning-point in saving the patient's life. A case was related. Twenty grains of quinia were given after confinement, and ten grains daily until the sixth day. The patient was doing well, and the quinia was omitted. On the seventh day a violent chill came on, the temperature rose to 104° F., and in spite of large doses of quinia—45 grains in 12 hours—and thorough cinchonism, the fever and chills continued for three days. The cavity of the uterus was then cleansed by means of a probe and cotton, and the next morning, without the administration of quinia, the temperature was 103° F.; twelve hours afterward 102° F. The uterine cavity was cleansed three times, and the patient made a good recovery. The quinia was continued in the treatment, but not in very large quantity.

In a case of placenta prævia, the temperature, after delivery, rose to 104° F. It yielded to quinia, but the elevation returned, and symptoms of septicæmia followed. The case was treated by cleansing the uterine cavity, in addition to the use of quinia. The point to which he wished to direct especial attention was that when the elevation of temperature persisted in spite of the use of quinia, although there may be no tenderness of the abdomen, and many of the ordinary symptoms of septicæmia may be wanting, we should cleanse the uterine cavity, and not take it for granted that it is not a case of septicæmia.

Dr. Wm. T. Lusk remarked that he had not had any personal experience in cases corresponding to the severer form of malarial manifestation in puerperal women, described by Dr. Barker; yet ever since Dr. Barnes read his paper before the American Gynæcological Society, drawing attention to the fact that women living in a malarious region were subject to severe attacks of malarial fever immediately after confinement, his attention had been turned in that direction, and he had watched such cases with a great deal of interest. Within the last three months he had had three cases of the milder manifestations of malarial poisoning after confinement; chill, high fever, profuse perspiration, and all yielded readily to quinia. He regretted that post-mortem data had not been obtained regarding the severer forms. He thought that in some of the cases described by Dr. Barker septic symptoms were superadded, and aided in making them exceptionally severe. In the Maternity Hospital, when the report was made by the House Staff that some of the women had severe chills and high fever, followed by profuse perspiration, and that yielded readily to quinia, he always knew that it was only the forerunner of something more serious, and therefore a warning. If the women were not removed, peritonitis and cellulitis began to appear in other cases; and if still the warning was not heeded, there came an outbreak of puerperal fever. He could not but think that, at the bottom of the severe cases at least, there was some other poison than malaria, differing only in intensity, perhaps in the manner in which it enters the system, rather than in the quality and character of the poison itself, and capable of producing puerperal fever. He thought we should be on the alert in making a distinction between this form of poison and that which produces true malarial puerperal fever.

Dr. Barker suggested that it was not convenient to obtain post-mortem data unless patients die. Only one of the seventeen cases he had seen had terminated fatally, and in that one the question of septicæmia was thoroughly discussed by Dr. Hall, Dr. Thomas, Dr. Metcalf, and himself, and all were unanimous that there was not the slightest ground for suspecting septicæmia.

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*Origin and Prophylaxis of Sore Nipples.*

Professor Kehrer, of Giessen, considers (*Beitrag zur klin. und experimentellen Geburtsh. und Gynäkologie*, Bd. ii. Heft 1) a knowledge of the mechanism of sucking to be necessary to the proper understanding of the mode of production of sore nipples. First, the nipple is seized and drawn into the mouth, without, if the tongue is of normal length, the lower jaw touching the nipple. Second, the nipple again becomes shorter, and is drawn back out of the mouth. The well-known lesions of the nipple (chaps, etc.) result from the squeezing of the nipple between the jaws, its rubbing against the hard gums, and its being wetted with milk, and with the secretions of the mouth. The author considers, and etiologically differentiates, vesicular excoriations, semicircular lateral ulcers, erosions at the summit of the nipple, fissures, etc. The conditions upon which the origin of such depends are the power, frequency, and duration of sucking, and the physical peculiarities of the nipple. According to most authors, primiparæ are more liable to these affections; and Kehrer does not agree with Winckel,

according to whom fissures are found as often in pluriparæ as in primiparæ. He has sifted his data, taken from lying-in women both in town and country, and gives for each the number of children born, the number of children suckled, the frequency of fissures, and of inflammation of the breast. He finds that these breast affections are so frequent that half of all women in some confinement suffer from them. Breast affections, especially chaps, are 20 per cent. more frequent in town than in country patients. They are, of course, more frequent in first lactations; fissures being in these twice as frequent, and mastitis three times as frequent as in others. The greater liability of town patients to sore nipples results from their smaller quantity of milk, badly-developed nipples, thinner skin, greater vulnerability, etc. For prophylaxis the author recommends the improvement of the nutrition of suckling women, to produce more abundant milk, and the drawing out of the nipple during pregnancy, for which purpose the author supplies his midwives with a suitable glass. The early application of an India-rubber shield to the nipple is of especial importance. During 1877, out of 122 suckling women, 22.95 per cent. suffered from sore nipples; while in 1878, out of 103 who suckled from the beginning with nipple shields, only 6.6 suffered in the same way. The shields, however, should not in practice be used for every case from the beginning, but only where there are small flat nipples, irritable skins, etc. The author further recommends the application of compresses soaked in glycerine. He thinks that hardening of the nipple during pregnancy by astringent applications is of doubtful utility as a prophylactic.—*Obstetrical Journal of Great Britain*, Jan. 15, 1880.

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*Condition of the Cervix in the Non-Pregnant Uterus.*

Prof. BANDL contributes to the *Archiv für Gynäkologie*, Bd. xiv. S. 237, an interesting paper on this subject. After a somewhat prolonged historical review of opinions on the question of the relation of the lower uterine segment to the cervix uteri during pregnancy, the author proceeds to maintain that in the virgin uterus the cavity of the cervix, as indicated by the mucous membrane, extends much higher up than is the case with women who have borne children, being traceable to nearly the middle of the body of this organ; that during the first pregnancy the upper portion of that cervical cavity becomes gradually transformed into a portion of the body of the uterus, so as to amplify the lower uterine segment, its mucous membrane being changed into a true decidual membrane. Over this surface the ovum is adherent. In subsequent pregnancies the mucous membrane of this upper portion of the cervical cavity never regains the arrangement and characters of a cervical mucous membrane. According to Bandl, therefore, in the case of a woman who has borne children, what he calls the ring of Müller, which is ordinarily called the internal os uteri, is not the true internal os, but the lower extremity of this upper permanently transformed part of the cervix. The portion indicated is separated from the rest of the cavity of the body, he asserts, by a distinct narrowing of the uterine walls. According to Bandl, therefore, when the cervix is fully dilated during delivery, the cavity which is ordinarily represented by the fully dilated cervix, as in Braune's plates, is not that part of the uterus bounded superiorly by the ring of Müller and inferiorly by the outer os, but this together with the altered upper portion. He maintains that the original inner os may always be detected, in the case of a woman who has borne children, high up above the ring of Müller, and near the middle of the cavity of the body of the uterus. Some drawings are produced in support of this somewhat extraordinary view.—*Edinburgh Med. Journal*, Feb. 1880.

*Dilatation Instruments in Gynæcological Practice.*

In No. 26, *Centralblatt für Gynäkologie* 1879, Prof. CARL SCHROEDER asks the question, "Are dilatation instruments necessary in gynæcological practice?" in reference especially to a paper in No. 25 of the same Journal by Fritsch, advocating rapid dilatation under chloroform with steel sounds of gradually increasing size, instead of sponge or tangle tents. Schroeder goes further, and would do away with dilatation in all cases. He proposes, in most cases where dilatation is ordinarily advised, to remove from the interior of the uterus, for examination, a small portion of the mucous membrane through the undilated cervix, by means of a sharp curette with appropriate long flexible handle. If found necessary, the hypertrophied mucous membrane may be entirely removed in this way. If it is really necessary to get the finger into the cavity of the uterus, Schroeder proposes to divide on both sides the vaginal portion of the cervix throughout its entire length, and then to pass the finger through the inner os, which in such cases is usually large, by forcible pressure of the index finger. Counter-extension is made either by pressure upon the fundus of the uterus, or by traction on the neck with a volsella. The incisions made in the vaginal portion of the cervix Schroeder unites by suture so soon as the necessary operative interference with the cavity of the uterus is effected. [It appears to us that few people will regard the advantages presented by the heroic methods either of Fritsch or of Schroeder superior to the ordinary method of slow dilatation by the use of tents.—A. M.]—*Edinburgh Med. Journal*, Feb. 1880.

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## Medical Jurisprudence and Toxicology.

*Effects of Poisoning by Cantharidin.*

M. V. CORNIL has recently made a series of investigations on the lesions of the kidney and bladder in rapid poisoning by cantharidin. He finds that cantharidin, the action of which is simultaneously manifested on other organs of the body, causes in the first instance in the kidney almost immediately after its introduction beneath the skin the diapedesis, or escape of both white and red corpuscles from the vessels constituting the glomeruli; and in addition, there appears an impregnation and swelling up of the cells lining the capsules of Malpighii, and of the *tubuli contorti*, by a liquid containing hematic granules. After a short space of time inflammation of the *tubuli recti* and collecting tubes is observed characterized by a modification of the form of their cells, which, instead of presenting their usual definite form, become variable or irregularly polyhedric, and this state is very general in the cells of other organs of the body. The bladder, after the first act of micturition, in which a large quantity of urine is discharged, and which occurs about fifteen or twenty minutes after the animal has been poisoned, remains contracted. Its surface is red, it contains a few drops of turbid urine with leucocytes, and very large cells, which may either be spherical or elongated and flat. An hour after the injection of the poison these large cells, which contain from two to eight or ten round nuclei, are either floating freely in the urine, or are adherent to the surface of the mucous membrane, and about to detach themselves from it. The elongated cells are irregular at their borders, and present rounded processes. Their protoplasm is granular, solid, homogeneous, stains yellow with picrocarmin, whilst the nuclei become red. Nuclei may also be seen about to divide, or several smaller nuclei may be seen near a larger one.—*Lancet*, Feb. 14, 1880.

# MEDICAL NEWS.

*Colour-blindness among Railroad Employés.*

The Massachusetts Railroad Commissioners, in their Annual Report, consider the subject of colour-blindness among railroad employés, and the necessity of legislation on the subject. They state that in this country examinations have been made by many railroad companies since attention has been called to this subject. Dr. Keyser, of the Wills Hospital, Philadelphia, is reported as having examined all the employés on all the roads terminating in Philadelphia, except the Pennsylvania Railroad. His report was that 3½ per cent. "have defects of such a character as to make them really incapable and unsafe to fill the positions they occupy." He found, also, that an engine-man who was green-blind never mistakes as to the colour of signals. In Maine, an examination, heretofore referred to, was made on the Maine Central Railroad, with results not unlike those of Dr. Keyser.

The final conclusions of the Board of Commissioners are: 1. That the existence of colour-blindness, total and partial, is a well-established fact, and that there are men who, by reason of such defect, are unfit for positions on railroads requiring ability to distinguish colour signals. 2. That the extent of dangerous colour-blindness, *i. e.*, such colour-blindness as unfits persons for railroad employment, has been greatly exaggerated, and that a very small per cent. of persons are, for this reason, unfit for such employment. 3. That examination may be properly made by persons not medical experts; and that such examinations will certainly be sufficient, if doubtful cases are referred to such experts. 4. The Board recommends that every railroad company shall have an annual examination of every employé whose duties require or may require capacity to distinguish form or colour-signals, and that no one shall be so employed who has not been thus examined. The examination should refer to colour-blindness, and to other defects in vision. It should include all who are in any way concerned in the movement of trains.

The Board does not recommend any legislation on the subject, believing that self-interest will make railroad managers careful to have examinations as to colour-blindness among employés made, so as to avoid even false charges that accidents have resulted from the existence of such defects.

Dr. B. Joy Jeffries has recently appeared before the Railroad Committee of the Massachusetts House of Representatives in opposition to the conclusion of the Commissioners that the testing for colour-blindness among railway employés may be safely entrusted to a non-medical man, and showed in great detail how laymen might be easily misled in making these examinations. He stated that in Europe railroad officials were first directed to make these examinations, and had utterly failed in finding who were and who were not colour-blind or had defective vision; and he read letters from Prof. Donders and Prof. Holmgren strongly urging that medical experts only could be safely employed in testing colour perception and acuteness of vision.

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*Rapid Cure for a Cold.*—R. RUDOLF reports, in the *Gazetta Medicin Italiana*, the following observation made on himself. Being seized with a severe coryza,

he happened to chew one or two twigs of eucalyptus, at the same time swallowing the saliva secreted, which had a bitter and aromatic flavour. To his surprise he found that, in the course of half an hour, the nasal catarrh had disappeared. Some days latter, he was seized with another attack from a fresh exposure to cold, when the same treatment was followed by an equally fortunate result. He then prescribed the remedy to several of his patients, all of whom were benefited in the same way. He believes that this treatment is only suitable in acute cases.—*Brit. Med. Journ.*, Jan. 24, 1880.

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*Bigelow's Operation for Stone.*

In a recent number of the *Lancet* (Jan. 10, 1880) Mr. THOMAS SMITH, Surgeon to St. Bartholomew's Hospital, publishes an abstract of a clinical lecture on litholapaxy in which he says of this operation, that it "is a new one, founded on principles different from those which have hitherto guided us in the performance of lithotrity, and it is possible, or, rather, highly probable, that it will entirely reform our practice in the treatment of stone in adults." In proof of the statement that it is new in principle he offers several quotations from Sir Henry Thompson's recent writings.

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*Graduates in Medicine in* 1880.

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| Jefferson Medical College, Philadelphia | 196 |
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| University of Louisville | 95 |
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| Nashville Medical College | 67 |

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*Boracic Acid in Eye Inflammations.*

Dr. SAMUEL THEOBALD, of Baltimore, recommends (*Med. Record*, Feb. 7, 1880) the use of a solution of boracic acid (two to four grains to the ounce) in conjunctival inflammations.

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*Who Founded the National Medical Library?*—For many years there was a small collection of medical books and journals in the Surgeon-General's office at Washington, which collection was commenced by Surgeon-General Lovell prior to 1836. At the commencement of the War it amounted to between 300 and 400 volumes. During the period of sixteen months ending Aug. 30, 1863, that Dr. Hammond was Surgeon-General, 359 volumes were purchased for the use of the office, the principal works being sets of two journals. During the years 1864 and 1865 about 1000 volumes were purchased, mainly works selected by Drs. Woodward and Otis. When the library came under the charge of Dr. Billings, in the autumn of 1865, it contained about 1800 volumes. At present,

under the intelligent and energetic management of the librarian, Dr. Billings, it contains about 50,000 volumes and 60,000 pamphlets.

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*Meetings of State Medical Societies.*—The following State Medical Societies will hold their annual meetings next month:—

Kentucky State Medical Society, at Lexington, May 4th.
Kansas State Medical Society, at Leavenworth, May 11th.
Michigan State Medical Society, at Grand Rapids, May 12th.
Illinois State Medical Society, at Belleville, May 18th.
Indiana State Medical Society, May 18th.
Missouri State Medical Society, at Carthage, May 18th.
Pennsylvania State Medical Society, at Altoona, May 19th.
New Jersey State Medical Society, at Princeton, May 25th.

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*National Convention for Revising the U. S. Pharmacopœia.*—This Convention will meet at Washington on the 5th of May.

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*Sanitary Convention.*—The National Board of Health requests all quarantine authorities of the United States to send authorized representatives to meet with it in Washington on the 6th of May next, for the purpose of amending the rules and regulations prepared by the Board last year and recommended by it to State and local quarantine authorities for adoption.

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*A Substitute for the German "Sapo Viridis."*—Dr. GEO. HENRY FOX recommends (*Med. Record*, Feb. 14, 1880), as a substitute for the German "green soap," a soft olive soap used extensively in the manufacture of silk and other delicate fabrics. This he has tried and found to be a similar, though a far more elegant article than the ordinary sapo viridis. It is made from cold-pressed olive-oil, and owes its green colour entirely to the chlorophyl of the olive. As manufactured by Mr. Bagoe, Pharmacist, of New York, it is of unvarying alkalinity, wholly free from unpleasant odour, homogeneous, and, unlike the common green soap, it gives a perfectly clear solution with strong or dilute alcohol. He has found it advantageous to add to the soap a small percentage of glycerine, which, while counterbalancing the natural loss of free water, renders it a most agreeable preparation. The name "Sapo Olivæ Præparatus" is recommended for this soap.

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*The French Codex.*—The Minister of Public Instruction has designated a commission, with M. Gavarret as President, to revise and publish a new edition of the French Pharmacopœia. The last edition was published in 1867.

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*Naval Board.*—The Naval Medical Board of Examiners, which during the past seven or eight years has held its sessions in Washington, has been transferred back to the Naval Hospital at Philadelphia.

It consists of Medical Directors Edward Shippen (President) and George Peck, and Surgeons Newton L. Bates and Thomas N. Penrose. It convened March 1st to examine candidates for promotion and for admission.

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*Test Pellets.*—Dr. PAVY recently introduced to the notice of the London Clinical Society a new form of test for sugar. He considered the cupric test by far the most reliable, and this view stood in accord with that which was generally entertained by analytical chemists. It had been hitherto the practice to employ

the test in the form of a solution, prepared by mixing an alkaline tartrate with sulphate of copper and either potash or soda. When freshly prepared this solution fulfils all that can be desired; but there is the disadvantage belonging to it that, after being kept for some time, especially if exposed to light and air, it is liable, without the presence of sugar, to throw down a certain amount of reduced oxide on boiling, and thus possibly to mislead unless precautions are taken to provide against it. Dr. Pavy had long felt that it would be very desirable if the ingredients of the test could be incorporated and kept in a solid form; and some years ago he made a fruitless attempt to attain this object. Latterly he had given his attention afresh to the subject, and the test pellets shown were the result of this. They contained the solid ingredients of cupric test solution in a dry state, and they were brought into the condition of a coherent mass by compression. The preparation had been placed in the hands of Mr. Cooper, of Oxford Street, who most successfully met the difficulties that were unexpectedly encountered. There appeared no reason why the pellets should not keep for an indefinite time without undergoing change if preserved in a closed bottle away from moisture. In using them all that was necessary was to place one in a test-tube with about 3 cc., or rather under a drachm, of water, and to apply heat until complete solution had occurred. It formed a clear, deep-blue liquid, which actually constituted the cupric test solution. At present it was only as a qualitative test that the pellets were introduced, but there was no reason that hereafter the preparation should not be conducted with sufficient attention to accuracy of weighing to render them applicable for quantitative analysis.—*Lancet*, Jan. 31, 1880.

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*Liability of Hospitals in Malpractice Suits.*—The Supreme Court of Rhode Island, according to the *Maryland Med. Journal* (Dec. 1879), has recently decided that hospital corporations should be considered liable for failure to exercise reasonable care in selecting skilful, competent men as internes. This decision grows out of a case where suit for malpractice was instituted against a Rhode Island Charity Hospital, by a patient whose fingers were cut off by a circular saw. Hemorrhage was excessive, and was only controlled by the use of the tourniquet, which instrument was kept on for seventeen hours. The result was, the arm was amputated at the shoulder joint, the patient affirming that careless treatment, upon the part of the interne, had induced this result. The court directed the jury to give a verdict for the defendant, on the ground that a charity institution should not be made liable for negligence or unskilful treatment. The case was taken to the Supreme Court, with the above decision.

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Obituary Record.—At Dublin, on the 1st of February, aged 77, Sir Dominic John Corrigan, Bart., M.D., Physician-in-Ordinary to the Queen in Ireland.

Sir Dominic was the last representative of the great physicians and surgeons who, by their writings and oral teachings during the second and third quarters of this century, made the Dublin School of Medicine one of the most famous in Europe. His most meritorious writings were an article on Permanent Patency of the Aortic Valves, which led Trousseau to give to the disease the name of "Maladie de Corrigan," his description of Cirrhosis of the Lung, and his lectures on Fever.

—— On the 1st of March, at his residence near Baltimore, William Maxwell Wood, M.D., late Surgeon-General U. S. Navy. Dr. Wood was appointed Chief of the Bureau of Medicine and Surgery in 1870, and was retired the following year on account of age.

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